

# THE NOVEMBER SCIENTIFIC MONTHLY

EDITED BY J. MCKEEN CATTELL

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# THE SCIENTIFIC MONTHLY

NOVEMBER, 1930

## OUR RAINFALL: HOW IS IT FORMED AND WHAT BECOMES OF IT?

By Dr. GEORGE FRANCIS McEWEN

THE SCRIPPS INSTITUTION OF OCEANOGRAPHY, LA JOLLA, CALIFORNIA

### INTRODUCTION

ATMOSPHERIC moisture is carried from the ocean over the land, precipitated in the form of rain or snow, returned as run-off to the ocean and evaporated into the air, thus completing the "water-cycle."

The distribution of water in the atmosphere, the ocean and the land is a momentary aspect of cyclical change. Before considering details of the processes involved in this water cycle some average figures will be presented to indicate the occurrence and distribution of water in the sea, the atmosphere, over the earth's surface and within the geological strata. Some idea of the amount of water in the ocean is revealed by the fact that the salt, if separated from the water, would form a layer nearly fifty yards deep over the whole surface of the earth. The volume of water in the sea is eleven times that of all land above sea-level and is approximately three hundred and twenty-seven million cubic miles, or one eight hundredth that of the earth. While water occurs mainly in the ocean it permeates the atmosphere in the form of vapor, clouds and fog. In addition to its visible occurrence in lakes and reservoirs, large amounts are distributed throughout the soil even down to a depth of thousands of feet in some re-

gions. Using the volume of water in the ocean as a convenient unit a general idea of the average amount of water distributed in these various ways is presented by the following table:

Volume of water in the ocean	1.0
Volume of water in the soil (ground water)	0.005
Volume of water in all inland seas and lakes	0.00009
Volume of water in the atmosphere	0.000009

But the condition is not static. From exposed water and land surfaces there is continual evaporation into the air, where water exists in the form of vapor and is carried in accordance with the atmospheric circulation to be condensed into fogs or clouds, from which a part is precipitated as rain or snow. After precipitation a large proportion returns as run-off to the original source, the ocean, thus completing the water cycle of transformation and transportation.

### TRANSPORTATION OF WATER VAPOR FROM OCEAN TO LAND

In considering certain details of the different processes that constitute the water cycle let us deal first with the atmosphere and limit our study mainly to the region about the Pacific Ocean. Averages of many thousand observations of wind and barometric pressure indi-



WINTER SCENE IN THE HIGH SIERRAS

MUCH OF THE WATER FOR POWER, IRRIGATION AND DOMESTIC USE COMES IN THE FORM OF SNOW WHICH ACCUMULATES AT HIGH ELEVATIONS.



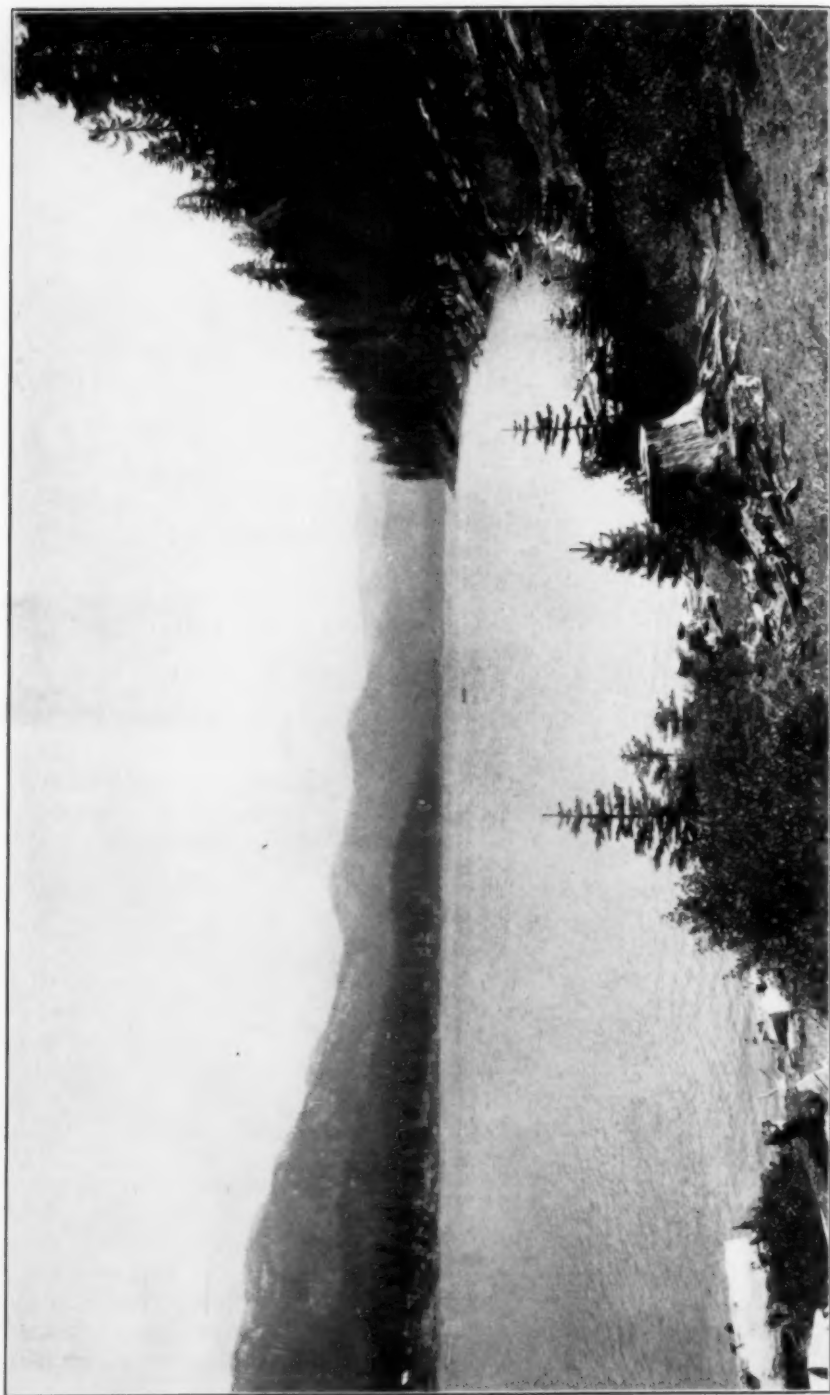
#### DETERMINING THE PROBABLE WATER EQUIVALENT OF SNOW

SNOW SURVEYS ARE CONDUCTED EVERY YEAR IN ORDER TO ESTIMATE THE PROBABLE VOLUME OF WATER TO BE EXPECTED WHEN THE SNOW MELTS. THIS SCENE TAKEN AT COTTONWOOD LAKE IN THE HIGH SIERRAS SHOWS THE INSTRUMENTS USED.

cate a high pressure area about a thousand miles west of San Francisco and a low pressure area over the Aleutian region. Winds blow out spirally from the high in a clockwise direction and blow in spirally toward the low in a counter-clockwise direction. From late winter to summer the high moves northward and farther from the coast. It also increases in intensity and area and the prevailing winds become steadier and stronger. The Aleutian low is especially well developed in winter and disappears in summer. South of the equator the winds are clockwise about a low and counter-clockwise about a high. A chart of average winds indicates a line of "convergence" (a line toward which air flows from both sides, usually accompanied by a flow parallel to the line) between the high and low. It is directed toward Vancouver in winter and toward central Alaska in summer. An equatorial line of convergence in the doldrum region is equally prominent, and lies

between the two oppositely circulating whirls about the highs of the North and South Pacific. The whole system is subject to a seasonal shift of several hundred miles to the north in our northern summer and to the south in winter. Along both the North and South Pacific coasts of America is a large cold water area between the high and the coast which extends seaward and toward the equator. These areas are especially cold and extensive when the highs and their accompanying winds reach their greatest development. A reduction of as much as  $15^{\circ}$  F. below the normal temperature for the latitude is occasionally found.

Certain outstanding characteristics of the precipitation of the eastern North Pacific coastal region will now be considered in relation to these atmospheric and oceanic conditions. As a general rule on-shore winds are accompanied by precipitation, while off-shore winds are dry. Polar winds are dry, and winds blowing from lower latitudes into higher



HUNTINGTON LAKE

THIS IS AN IMPORTANT STORAGE RESERVOIR IN THE BIG CREEK SYSTEM OF THE SOUTHERN CALIFORNIA EDISON COMPANY. IT IS LOCATED ABOUT 200 MILES NORTH OF LOS ANGELES AT AN ALTITUDE OF 7,000 FEET IN THE SIERRA NIVADA MOUNTAINS.



MOUNTAIN STREAM

FED BY MELTING SNOW AND FLOWING INTO HUNTINGTON LAKE

bring much moisture. Lines of convergence have abundant rainfall.

The Pacific Coast State of Colombia in South America has rain at all seasons and is in the doldrum line of convergence. Farther northwards, along the coast from Panama to Guatemala, about 90 per cent. of the precipitation falls during the six-month period from May to October. The northern Mexican frontier located in the off-shore trade-wind region is practically arid throughout the year, and north of this region extending as far as Vancouver Island the winter is the rainy season, corresponding to the changes in the high and low. The coasts of British Columbia and Southern Alaska have much rain throughout the year but most of it falls in winter when the frequency of cyclonic storms is greatest.

In addition to these regular seasonal variations there are differences from

year to year. Occasionally very great departures occur and can be explained by corresponding changes in the ocean. A striking example of direct ocean influence upon coastal weather is afforded by the westward deflection of the Humboldt current in 1925, which permitted the warm equatorial waters to penetrate southwards along the coast of South America. This unusual development of the warm counter current, El Niño, resulted in abnormally heavy rains along the arid coasts of Ecuador, Peru and Chile. At the same time there was a corresponding deviation of the cold Benguela current, thus permitting the warm waters of the Guinea current to penetrate southward along the west coast of Africa and cause torrential rains along that arid coast. There is some evidence of a recurrence of a similar phenomenon about every thirty-five years.



SYPHON IN THE LOS ANGELES MUNICIPAL AQUEDUCT

THIS SYPHON IS A PART OF THE AQUEDUCT THAT SUPPLIES THE LOS ANGELES DISTRICT WITH WATER FROM OWENS LAKE 240 MILES DISTANT. THE CARRYING CAPACITY IS ABOUT 500,000,000 GALLONS PER DAY.

#### CONDITIONS ESSENTIAL TO PRECIPITATION

On the average there are about five parts of water vapor per thousand parts of air by weight, and the higher the temperature the greater is the possible amount of atmospheric water vapor. This invisible vapor when cooled sufficiently condenses, forming fogs or clouds. Minute particles, ions, dust, etc., which are widely distributed throughout the atmosphere, act as nuclei which greatly facilitate this condensation. The densest clouds contain about five grams of water per cubic meter of air. An average amount would be near one gram of water per cubic

meter of air or about one part in a thousand by weight. Further cooling is necessary to produce precipitation. These drops of water may be as much as two or three millimeters in diameter. Accordingly conditions essential to precipitation are primarily conditions that cool the atmosphere, which is a mixture of air and water vapor. This cooling may be brought about by mixture with relatively cold air, by a flow toward regions of higher latitude where a relatively lower temperature prevails, or by rising and expanding as the pressure decreases. In the atmosphere the latter process of "adiabatic expansion" is by

far the most important cooling agency producing clouds and precipitation. Accordingly, it is important to study the conditions that cause a rapid rise of air.

#### OROGRAPHIC AND CYCLONIC RAIN AND THE POLAR FRONT THEORY OF STORMS

A very good example of certain conditions that cause rain is afforded by the warm moist ocean winds blowing to the northwest from the Indian Ocean and turning to the northeast across the Arabian Sea and the Bay of Bengal during the northern summer. These monsoon winds pass over a warm ocean path some four thousand miles in length and are saturated with moisture at a relatively high temperature. Wherever they are directed against steep slopes of coastal mountain chains in India there is heavy precipitation, called orographic because of the topographic factor. The air is deflected upward along these slopes with sufficient velocity to produce a rapid fall in temperature which causes precipitation. Where the winds descend

over the warm land surface the air takes up any available moisture and precipitation is impossible.

If the air current is stable (density decreases at a greater than average rate as the height increases), as it is forced up a slope the surrounding air is lighter and thus ascent is resisted. The air will tend to flow out around obstacles rather than to continue upward. This explains why oftentimes the heaviest precipitation does not occur at the top of a mountain, but at some intermediate elevation. Air warmed from below, as often happens in winter when the air is colder than the sea, is unstable, and orographic rainfall is thereby aided. On the other hand, when air is cooled from below, as is often the case in summer over the cold inshore water belt of our California coast, a local fog or slight drizzle results, but orographic rain is hindered.

A different but very important condition producing a rapid updraft of air independently of local topography often develops at a surface of discontinuity, separating warm and cold air at the



A MUNICIPAL HYDROELECTRIC POWER PLANT OF LOS ANGELES  
POWER IS DERIVED FROM THE FALL OF WATER WHICH LATER FLOWS INTO THE CITY'S LOCAL  
STORAGE RESERVOIRS. THIS PLANT IS LOCATED IN SAN FRANCISQUITO CANYON.

same level. By means of a very close network of stations in Norway, Professor Bjerknes found that warm winds converging toward colder winds rise above the colder air and their moisture is condensed just as if the warm air were rising up mountain slopes. There is convincing evidence that polar and equatorial air meet at a surface of discontinuity which intersects the surface of the earth and sea in a wavy line extending entirely around the earth at about  $50^{\circ}$  north latitude but departing hundreds of miles on either side of this average position. High and lows result from the separation of loops forming in this line of discontinuity or "polar front."

The weather of the northern hemi-

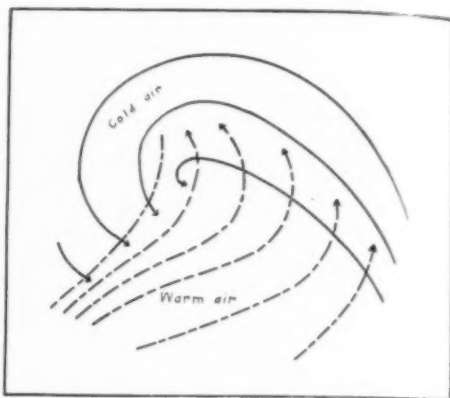


FIG. 1. MECHANICS OF MOVING CYCLONES

THE MOVING CYCLONE CONSISTS ESSENTIALLY OF TWO OPPOSITE CURRENTS—COLD ———, AND WARM ———.

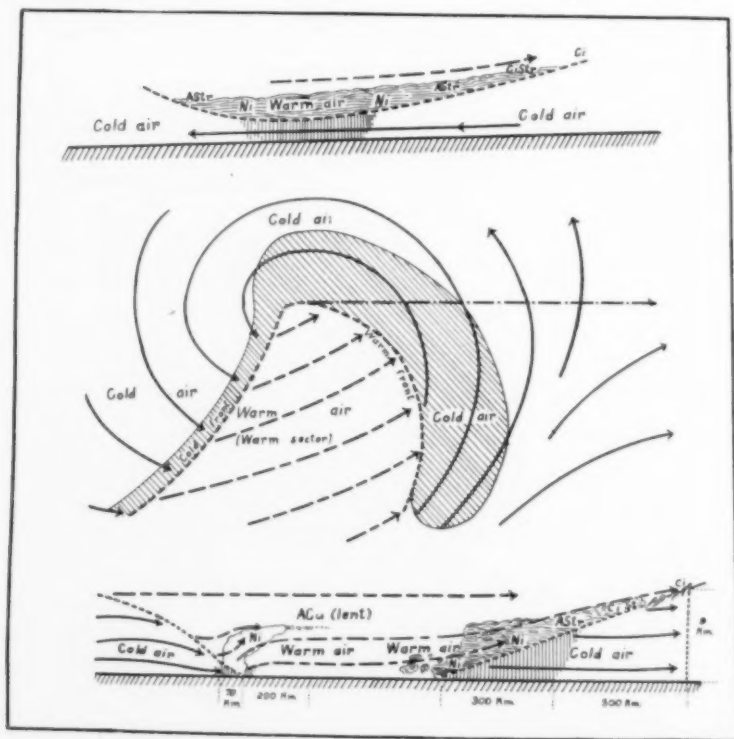


FIG. 2. PRINCIPAL FEATURES OF MOVING CYCLONES

AN AIR MASS OF COLD ORIGIN IS SEPARATED FROM ONE OF WARM ORIGIN BY A DISTINCT BOUNDARY SURFACE ——— THROUGH THE CENTER INCLINED AT AN ANGLE OF  $0.1^{\circ}$  TO  $1^{\circ}$  TOWARDS THE COLD SIDE. CHARACTERISTIC CLOUD TYPES, PRECIPITATION AREAS AND APPROXIMATE DIMENSIONS ARE SHOWN BY THE TWO VERTICAL SECTIONS. THE CYCLONE MOVES IN THE GENERAL DIRECTION OF THE WARM CURRENT AS SHOWN BY THE ARROW IN THE HORIZONTAL SECTION.

sphere is a consequence of advances and retreats of this polar front which marks the frontier between masses of air of different origin. The expulsion of great masses of polar air, forming anticyclones, is an essential element in the general circulation. Warm air flows along the surface of the earth and sea from the subtropic highs toward the polar regions, concentrating in warm tongues and continuing into the polar regions at the upper levels where it cools and later reaches the lower levels. Thus masses of cooled air accumulate behind the polar front and cause it to advance southward. Finally, at places of least resistance great masses of cold air break through and are expelled in the direction of the tropics. This results in a northward retreat of the polar front, after which the process is repeated. Moreover, this intermittent quality of the general atmospheric circulation is especially characteristic of the winter season when marked discontinuities of sea surface temperatures prevail.

A succession of cyclonic storms then develops from alterations of the polar front, and they travel from west to east along tracks which appear as lines of convergence in charts of monthly averages. The structure of one of these cyclones is essentially as follows (see Figs. 1 and 2). There is a warm sector of less than a quarter of the total area and lying to the south; this air flows in a counter-clockwise direction and is deflected upward over the cold air at the right (the observer is assumed to be facing to the north). The cold air also flows in a counter-clockwise direction and forces the warm air upward at the left of the warm sector. Both the boundaries of the warm sector are lines of convergence. According to hydrodynamic theory a line of convergence will move to the right of an observer looking along the line in the direction of flow. Thus there is an ascent of air at

both these lines and the cyclone moves from west to east. The spiral motion is due to the combined effect of the low pressure of the central area and the de-

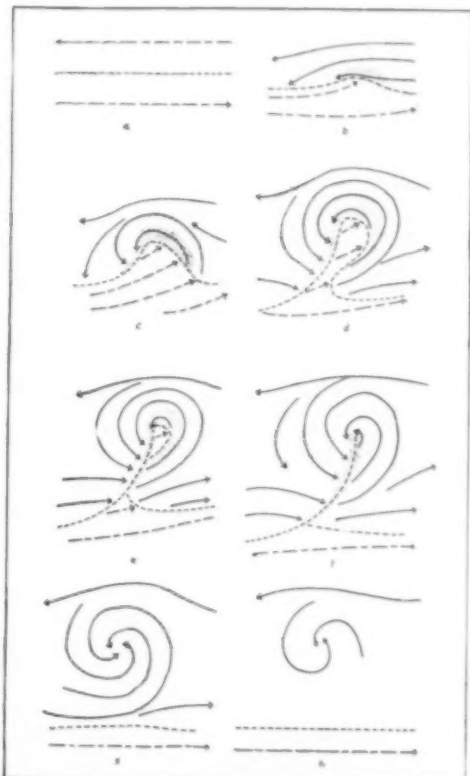
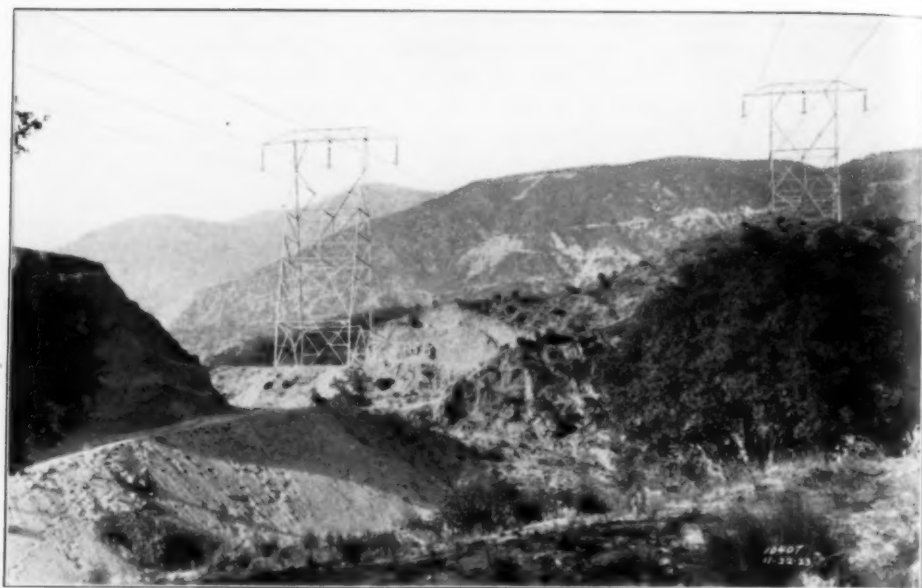


Fig. 3. HOW A CYCLONE DEVELOPS

THIS DIAGRAM SHOWS HOW A CYCLONE DEVELOPS FROM TWO OPPOSITELY DIRECTED (EAST AND WEST FOR EXAMPLE) AIR CURRENTS (A) OF DIFFERENT TEMPERATURES SEPARATED BY A NEARLY STRAIGHT BOUNDARY ---- BY A BULGING OUT TOWARD THE COLD SIDE, (B) AND FORMATION OF A WARM TONGUE (C) WHOSE TIP BECOMES THE CENTER OF THE CYCLONE. THIS WARM TONGUE IS THE WARM SECTOR (D) OF THE NEWLY FORMED CYCLONE WHICH IS PROPAGATED LIKE A WAVE ON THE BOUNDARY SURFACE BETWEEN WARM AND COLD AIR IN THE DIRECTION OF THE WARM CURRENT. THE AMPLITUDE (NORTH AND SOUTH) OF THE WAVE INCREASES, THE WARM TONGUE GROWS NARROWER AND IS FINALLY CUT OFF (D), (E), (F). THE CYCLONE THEN DEGENERATES INTO A VORTEX (G) WITH A COLD CENTER, ITS KINETIC ENERGY CHANGES INTO POTENTIAL AND THE CYCLONE DIES (H).



#### HIGH VOLTAGE POWER LINES

ELECTRICAL ENERGY GENERATED IN HYDROELECTRIC POWER PLANTS IN THE MOUNTAINS IS TRANSMITTED BY HIGH VOLTAGE LINES TO TRANSFORMER STATIONS.

fleeing force due to the earth's rotation. There is at first an interchange of warm and cold air causing a transformation of potential into kinetic energy which changes later into potential energy resulting in the death of the cyclone as indicated in Fig. 3.

Both the irregular and seasonal north and southward displacement of the polar front result in a corresponding migration of the storm tracks, and the position and intensity of the North Pacific high determines how far south these cyclonic storms reach the North American coast. Finally, the sea surface temperatures determined by solar radiation, evaporation and the varying ocean currents are a fundamental control of the highs. Thus we are led to regard the ocean and overlying atmosphere as parts of a complex mechanism driven by the sun's energy and obeying the laws of physics.

#### THE RETURN FLOW OF WATER FROM LAND TO SEA

Precipitation varies greatly both with respect to geographic location and time, and may be in the form of rain or snow. A combination of such variability with a corresponding variety of topographic features, types of soil and vegetation results in a great diversity of run-off. A part of the water collects in lakes or artificial reservoirs, some penetrates into the earth's surface forming ground water, some is evaporated and some flows directly into the sea. All these conditions raise important problems in forestry, agriculture and water-supply engineering.

The great economic importance of obtaining an adequate understanding of hydrology, which includes the variations in rainfall intensity and their causes and the relation between rainfall and run-off, is especially recognized by engineers.

Lack of such basic information has resulted in extensive inundations of highly developed valleys thought to be properly protected by flood control systems. Such disasters have awakened public recognition of the national character of this vital question of flood control. The importance of adequate records of rainfall and run-off and the coordination and interpretation of such data are further illustrated by two cases typical of the West.

In central Arizona where the rainfall and run-off habits are irregular and violent there is along Queen Creek a drainage area of 143 square miles having an annual precipitation of fifteen inches. The run-off is as high as 9,000 second feet at high flood, but the annual average is only fifteen second feet. In contrast to this condition the maximum run-

off from an equal area along Cedar Creek in northwestern Washington is 3,600 second feet, the lowest is 294 and the average is 1,089. This average is about 90 per cent. of the total volume from the precipitation of 120 inches per year. This region is characterized by steady, gentle and dependable rainfall, in contrast to the sudden, irregular, brief torrential downpours characteristic of a desert. Accordingly, in arid regions it is necessary to provide reservoirs of relatively great storage capacity to equalize the flow. Furthermore, in order to prevent loss of arable soil and to reduce the silting of reservoirs, the watershed should be provided with a suitable vegetation cover. This has the additional advantage of reducing the variability of the run-off.

The various problems thus suggested



ONE OF THE EDISON COMPANY'S HYDROELECTRIC PLANTS



INSULATED CLOSING WATER-BOTTLE AND THERMOGRAPH FOR RECORDING OCEAN TEMPERATURES

AFTER THE OPEN INSULATED WATER BOTTLE IS SENT DOWN TO THE DESIRED DEPTH, IT IS CLOSED BY RELEASING A WEIGHT WHICH SLIPS ALONG THE CABLE. THE TEMPERATURE IS MEASURED IMMEDIATELY AFTER HAULING THE BOTTLE UP OUT OF THE WATER. THE THERMOGRAPH (IN THE SHELTER) AT THE END OF THE SCRIPPS INSTITUTION'S PIER RECORDS BOTH SURFACE AND BOTTOM TEMPERATURES.

by purely economic considerations can not be solved without a proper basis supplied by investigations of the cause-and-effect relation between rainfall and run-off, and such studies must include the range of conditions influencing rate, sequence and continuity of the various natural processes involved in the rain-producing cycle. Other topics of importance are solar radiation, atmospheric circulation, evaporation, expansion, cooling condensation, deposition, percolation, stream flow and the accretions or losses sustained *en route*.

Owing to the interconnection of the oceans and the oceanic circulation the run-off from the land ultimately becomes distributed over the whole surface of the sea. Thus we are led to consider the process of evaporation which completes the water cycle.

#### EVAPORATION FROM WATER AND LAND SURFACES

Although complementary to precipitation and of equal importance there is much less dependable information about the rate of evaporation. This is because

the measurement of precipitation is direct and readily made, while evaporation must be determined indirectly from suitable observations made with the aid of special equipment. For example, direct observations from a pan must be corrected by special methods in order to obtain the evaporation from a neighboring lake or reservoir. Although many investigators have worked on the problem of determining the rate of evaporation from lakes and reservoirs discordant results have been reached, owing to inherent difficulties and to lack of time to carry out a thorough rational investigation of suitable technique. Empirical methods were used and there was neither general agreement as to the relation of pan evaporation to that of a large body of water nor as to the method of estimating evaporation from meteorological data. Even less satisfactory information has been obtained about ocean evaporation, owing not only to certain observational difficulties peculiar to a moving ship, but also to lack of a rational method of interpreting the observations. Available observations indicate a rate of about 2.7 feet per year from the sea. There is also evaporation from the soil which under different conditions varies from about 20 per cent. to 90 per cent. of that from a water surface.

In spite of the need of a clear understanding of the physics of evaporation long recognized by engineers, geologists, meteorologists and oceanographers, no investigation of the subject, leading to satisfactory results, had been carried out before 1920. Mr. Cummings, then assistant in physical oceanography at the Scripps Institution, undertook to make a critical study of the problem of evaporation. This investigation begun at the Scripps Institution led to cooperative work at the California Institute of Technology a few years later by Messrs. Cummings, Bowen, Richardson and Montgomery.



THERMOGRAPH FOR RECORDING  
OCEAN TEMPERATURES

A STEEL BULB AND CAPILLARY STEEL TUBE LEADING TO THE HOLLOW COILED SPRING ARE FILLED WITH MERCURY WHOSE VOLUME CHANGES WITH THE TEMPERATURE. THE LEVER ARM TERMINATED BY A PEN RECORDS THE TEMPERATURE. THIS INSTRUMENT IS PROVIDED WITH TWO SEPARATE ELEMENTS, ONE FOR THE SURFACE AND ONE FOR THE BOTTOM, BUT RECORDING ON THE SAME PAPER.

Since these investigations of the process of evaporation were based upon the principle of energy exchanges some average estimates are presented first. The rate at which radiant energy from the sun passes through a square centimeter of surface normal to the rays at the outer limits of the atmosphere has been estimated to be two calories per minute, or one fifth horse-power per square foot of normal surface. If this energy were all available at the earth's surface for evaporating water the rate would be one inch per twelve-hour day at the equator and about one half as much at mid latitudes. The actual rate averages nearer 0.1 inch per day. Solar radiation at the earth's surface is much reduced in passing through the atmosphere. Even when the sun is in the



INSULATED WATER-BOTTLE OPEN READY TO LOWER INTO THE WATER

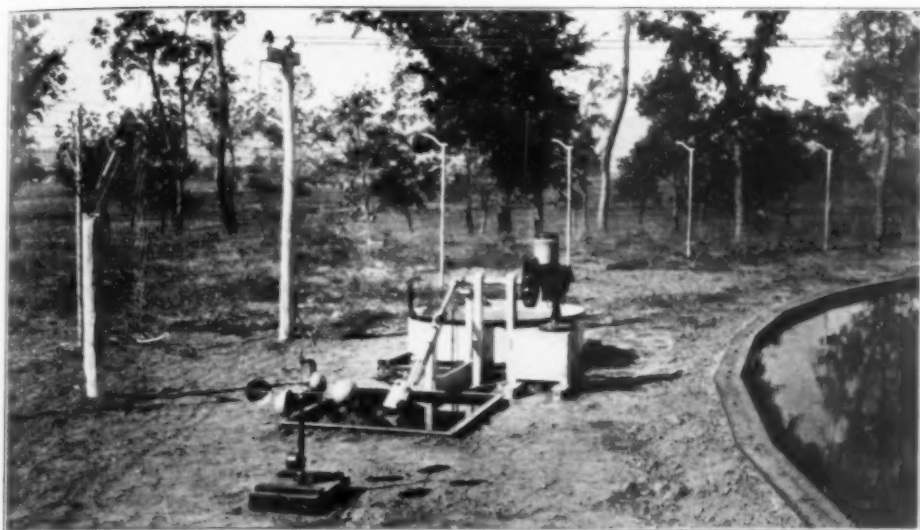
zenith, which position corresponds to the shortest path, and when the air is clear and dry there is a reduction to about 90 per cent. by absorption. Foreign substances, including water vapor in the air, and a departure from the zenith position reduce the amount still more, and dense cloud masses cut off all the radiation.

It is natural to regard evaporation from lakes as a process controlled by atmospheric conditions, such as air temperature, wind and humidity. This view, then shared by Cummings, was not supported by an extensive statistical analysis which he made for the purpose of isolating the effects of various factors influencing evaporation. In particular,

a very weak correlation was indicated between evaporation and humidity. He concluded that there must be a fundamental physical reason for this unexpected result.

Accordingly, it was decided to make a new beginning and attack the evaporation problem from the standpoint of the first law of thermodynamics. This investigation led him to the conclusion that any change in wind or humidity is always accompanied by a change in lake temperature which partially neutralizes the direct effect on the evaporation rate to be expected from the original change in atmospheric conditions, if the heat received from the sun remains constant.

Making use of the principle that the



## INSTRUMENTAL EQUIPMENT

FOR EVAPORATION MEASUREMENT, INCLUDING WIND VELOCITY AND RELATIVE HUMIDITY.

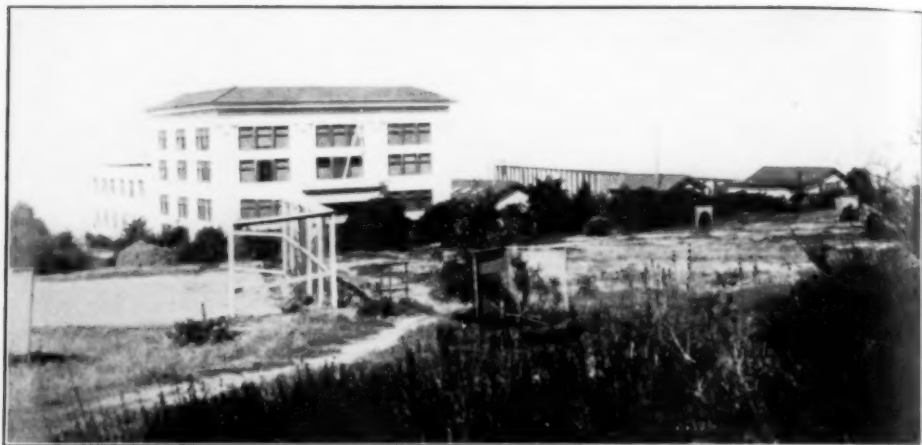
rate at which energy is removed by evaporation must equal the difference between the rate at which energy is supplied by solar radiation and lost by back radiation corrected for heat storage in the water, Dr. Cummings formulated an approximate equation for computing the evaporation from a lake. Further

improvement of the equation resulted from a theoretical investigation by Dr. I. S. Bowen, of the California Institute of Technology, on the ratio of sensible heat carried away by the wind and diffusing into the air to the latent heat carried off by vapor.

Extensive observations conducted by



SCRIPPS INSTITUTION'S PIER VIEWED FROM THE SOUTHEAST



SCRIPPS INSTITUTION'S LIBRARY, MUSEUM, LABORATORY BUILDINGS  
AND PIER

Dr. Cummings and Mr. Burt Richardson, using containers of various sizes, proved the correctness of the equation, thus modified, and led to a rational and accurate procedure for determining the evaporation from a lake.

During the progress of these investigations I was applying to the general problem of evaporation the principle of equating appropriate rates of energy at each level in the body of water. These investigations led to a mathematical formulation and solution of the problem of the downward diffusion of the surface cooling due to evaporation and back radiation and the heating caused by solar radiation. By means of the theory thus developed, the rate of surface cooling and the rate at which solar radiation penetrates the surface can be estimated from serial temperature and salinity observations. With the help of results outlined above the effect on surface cooling of factors other than evaporation can be eliminated. Thus one of the results of this cooperative undertaking is to provide a possible method of estimating evaporation from the sea.

#### CONCLUSION

While a good deal of information has been obtained regarding the water cycle there are many deficiencies as to detail, and there is room for much progress in theoretical studies carried on for the purpose of understanding the physical laws and interpreting the observations. Moreover, we have the fundamental instrumental designs and theoretical technique for making quantitative estimates of the processes that combine to make up the water cycle, but there is need of applying them to an extent commensurate with the importance of this vital world-wide phenomenon.

In the interest of the economy and safety of our public works appropriate observations should be made, having in mind the need of continuity as well as the great cumulative value of the length of records. All pertinent data should be assembled, coordinated, interpreted and made available. It is only by progress in this direction that engineers can solve the various problems that arise in working out the design of projects involving flood control or water supply.

# THE ANCIENT LIFE OF YUMA COUNTY, ARIZONA

By Dr. ROY L. MOODIE

SANTA MONICA, CALIFORNIA

A SEARCH for the evidences of disease in ancient times and among ancient creatures often leads me into interesting places. The scenes are visioned not only as they exist to-day but modified by the mental concept of what they were in antiquity, when the creatures we know now only from hard, dried bones were animals of flesh and blood, subject to influences to which living creatures are sensitive.

One fascinating thing about the study of paleopathology is that we never can tell what is going to turn up next—the investigation of which leads us into unsuspected realms of thought. We dream of such things as proving that those huge creatures, the sauropod dinosaurs found in the East African Tendaguru formations, are pituitary giants, though we will probably find the actuality to be something quite different and unsuspected.



FIG. 1a. THE FLOOR OF THE GILA RIVER

FIFTY-FIVE MILES EAST OF YUMA, IS MANY MILES WIDE, AND IS SUBJECT TO FLOODS, MANY YEARS APART. THE STREAM BED NOW LIES IN THE NORTHERN PART OF THE VALLEY, ITS COURSE BEING MARKED BY TALL COTTONWOOD TREES OF MANY YEARS' GROWTH. THE GILA RIVER VALLEY IS CUT INTO A HEAVY DEPOSIT OF LATE PLEISTOCENE DETRITUS, DERIVED FROM UPLANDS NOW DIFFICULT TO RECOGNIZE. THIS DETRITUS CONTAINS FOSSILS OF MANY KINDS WHICH ARE FAR REMOVED FROM THEIR ORIGINAL PLACE OF DEPOSITION, AND ARE SECONDARY DEPOSITS. ALL THE FOSSILS OBSERVED WERE WORN AS IF ROLLED BY WATER OR ERODED BY WIND. THE BONES ARE INTENSELY PETRIFIED, HEAVY AND WITH PEBBLES MOST THOROUGHLY CEMENTED TO THE SURFACES.

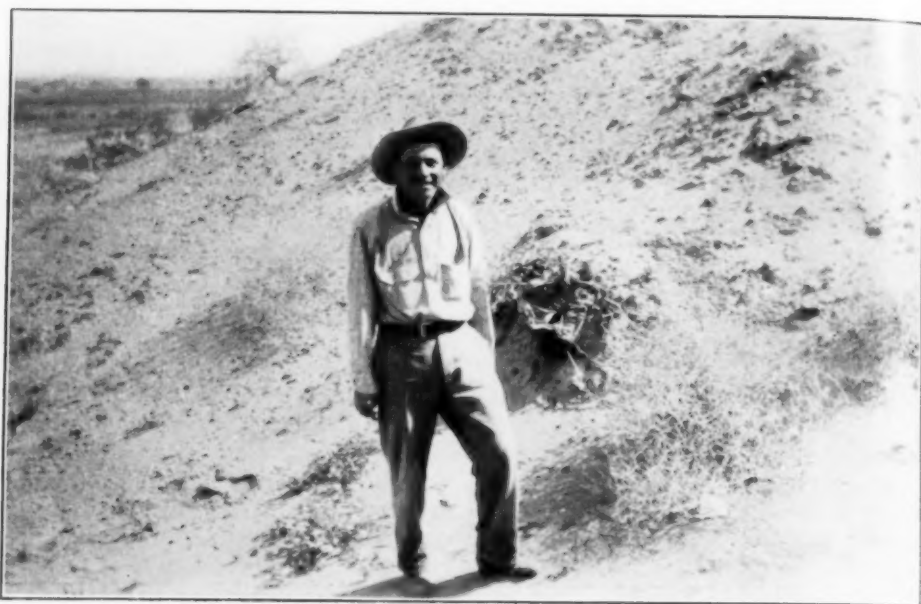


FIG. 1b. ON THE SOUTH RIM OF THE GILA RIVER VALLEY CHARLIE NORTON STANDS IN THE DEPRESSION FROM WHICH EIGHT YEARS BEFORE HE HAD SECURED THE DISEASED NECK BONES OF A MAMMOTH, SHOWN IN FIG. 3. THE DEPRESSION IS ALSO SHOWN IN FIG. 2, LEFT.



A GULLY

HAD YEARS BEFORE CUT A GAP INTO THE SOUTHERN RIM OF THE VALLEY, EXPOSING THE MAMMOTH BONES NEAR THE SPOT WHERE MR. JOHN DOAN STANDS. THE OUTWASH FROM THE GULLY IS IN THE FOREGROUND, UNDISTURBED BY FLOODS.



ANTELOPE HILL

IS A PROMINENT MONADNOCK THREE MILES WEST FROM THE SPOT WHERE THE MAMMOTH BONES WERE FOUND. THE RIM OF THE GILA RIVER VALLEY, IN THE FOREGROUND, IS FAIRLY STEEP AND ABOUT FORTY FEET HIGH.



—Specimen deposited in the Wellcome Historical Medical Museum, London

FIG. 3. COOSSIFIED NECK BONES

THE MOST INTENSELY DISEASED CONDITIONS ARE FOUND IN THE COOSSIFIED NECK BONES OF THE MAMMOTH: CERVICALS II TO VII. THIS IS THE SPECIMEN FOUND BY CHARLIE NORTON AT THE LOCALITY SHOWN IN FIGS. 1 AND 2. THE FOSSIL WEIGHS 57 POUNDS, WITH A GREATEST LENGTH OF 480 MM. THE MOST EVIDENT OF THE THREE PATHOLOGICAL CONDITIONS IS TO BE SEEN ON THE VENTRAL SURFACE OF THE BONES (*below*). THIS IS DUE TO THE COMPLETE AND EXAGGERATED OSSIFICATION OF THE VENTRAL LONGITUDINAL LIGAMENT. MEDICAL PEOPLE SPEAK OF THIS CONDITION AS *Spondylitis deformans*. CERVICAL ARTHRITIS, A PHASE OF RHEUMATISM, IS EVIDENT ON THE ARTICULAR SURFACE OF THE AXIS, SEEN TO THE LEFT. SO INTENSE IS THE DISEASED CONDITION THAT THE NECK BONES ARE PULLED OUT OF LINE, RECALLING THE WRYNECK OFTEN SEEN IN HUMANS.

A casual remark from a friend as to his having seen a curiously deformed series of cervical vertebrae of a mammoth led me to secure the loan of the specimen for study. Never had I seen such an object (Fig. 3), in which the pathological bone formed such a dominant amount of the structure. Immediately I saw the series of bones, thoroughly coossified, at once I knew that a scientific treasure had been brought to light. Closer study revealed a combination of difficulties, for the Pleistocene proboscidean not only had a stiff neck, but also a "wryneck," and rheumatism had so changed the articular surfaces of

the axis as to lead me to think that all seven of the neck bones were in life firmly attached to the skull by bony lesions, and that probably the rest of the backbone might show evidences of disease. A search for further skeletal parts would be thoroughly worth while.

Inquiry showed that the specimen was the property of Mr. John Doan, of Yuma, Arizona, who was holding it for educational purposes. He readily consented to go with us to the locality which he understood was a gravel pit being excavated for railroad grading. When we reached the place, some fifty miles east of Yuma, this proved not to be the case.

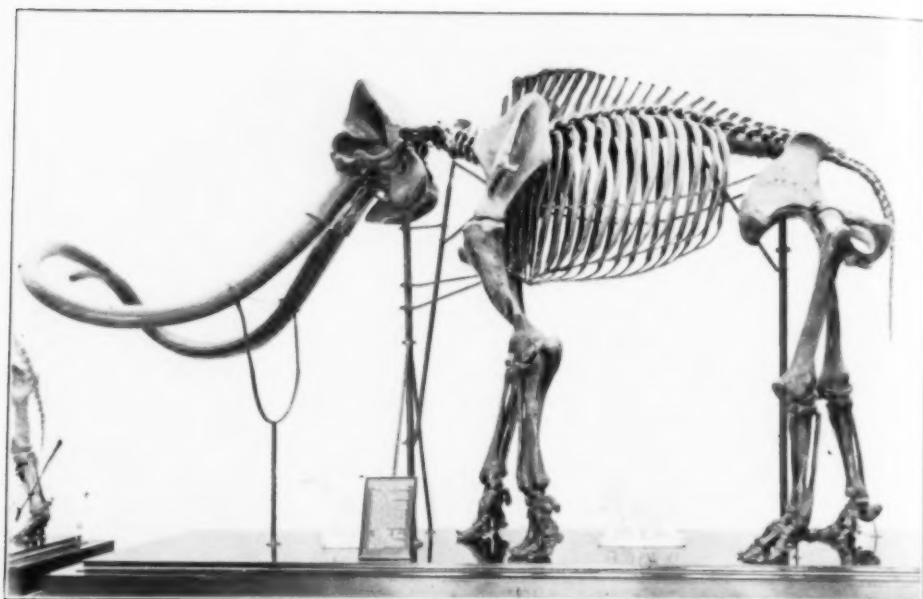


FIG. 4. A SKELETON OF A LARGE MAMMOTH

*Archidiskodon imperator* (LEIDY) FROM THE RANCHO LA BREA BEDS, AS MOUNTED IN THE LOS ANGELES MUSEUM. THE ANIMAL HAD A HEIGHT OF 13 FEET AT THE SHOULDERS. WHILE THERE IS NO ASSURANCE THAT THE DISEASED BONES SHOWN IN FIG. 3 BELONG TO THIS SPECIES, THEY WILL BE SO REGARDED UNTIL FURTHER EVIDENCE IS BROUGHT FORWARD.

In order not to lose too much time in undirected search along the escarpment of the Gila River Valley, we went in search of the discoverer, Charlie Norton, who was foreman of a group of men clearing brush in the valley near Roll. Since it was late in the day we arranged a meeting for early the next morning, and our party made a most delightful desert camp in the brush near Ralph's Mill on the automobile highway.

The next morning we made our way through the trackless brush to the notch (Fig. 2) in the escarpment, where eight years before Charlie Norton (Fig. 1) had discovered the cervical series which he had given to Mr. Doan. A few scraps of bone, evidently mammalian and possibly proboscidean, and a fragment of the plastron of a river tortoise, were discovered in the search made along the escarpment (Figs. 1 and 2). Whatever of the mammoth had been entombed in this secondary deposit had long since been removed.

Later at the roadside station Taena we found the upper two thirds of a very large mammoth humerus. The owner said he had seen it sticking out of the bank, evidently near the spot where the neck bones had been found. We heard also of the discovery of the greater part of a huge skull, some years before, which had been broken up for souvenirs. I saw also part of a shoulder-blade and part of the pelvis. I surmised that all these parts represented a single individual, and the size of the bones suggested an imperial mammoth, though we are still unable to prove this classification.

All evidence tended to show that the bones had been moved and secondarily entombed, since all the pieces which I saw were broken and rounded as if rolled by water for some distance.

The mammoths were the largest of the proboscideans, and there were several species which existed in North America during the glacial period. Remains of

the imperial mammoth (Fig. 4) have been found in the asphalt pits of the Rancho la Brea at Los Angeles. Pathological conditions are rare among the mammoths, though not uncommon among the smaller mastodons.

The mastodons and mammoths were companions during the Pleistocene, and it is not surprising that in the loose detritus of the upper plateau in the town of Yuma Mr. Doan found fragments of what seems to be a small mastodon (Fig. 6). Although the head of the proboscideans is huge, yet it is very light because of the immense number of air-spaces between the brain case and the outer skull table. Mastodons have previously been recorded from Arizona by Hay.<sup>1</sup> None of these shows disease.

<sup>1</sup> O. P. Hay, "The Pleistocene of the Western Region of North America and its Vertebrated Animals," Publication No. 322 B, Carnegie Institution of Washington. This records the following fossil animals from Arizona: Mammoths, pp. 25, 41, 45; mastodons, p. 10; ground sloth, p. 3; western horse, p. 54; pec-

The western horse (*Equus occidentalis*) was originally described by Leidy in 1865 from materials derived from Tuolumne County, California, and may be said to have been fairly common throughout the Southwest. Numerous elements of this horse, 15 hands high, from the Rancho la Brea deposits, in the Los Angeles Museum, show occasional evidences of disease. A complete study of these evidences is contemplated. All the material from Yuma County is healthy.

Of the camels and deer which were probably present we found no trace, although such remains were present in the placers.

A side trip to a petrified forest of undetermined age within a few miles of Yuma was full of interest. The logs and sticks were so thoroughly changed as to ring like steel when struck.

cary, p. 78; camel, p. 80; deer, p. 107; bison, p. 114. Only a small number are known from Yuma County.

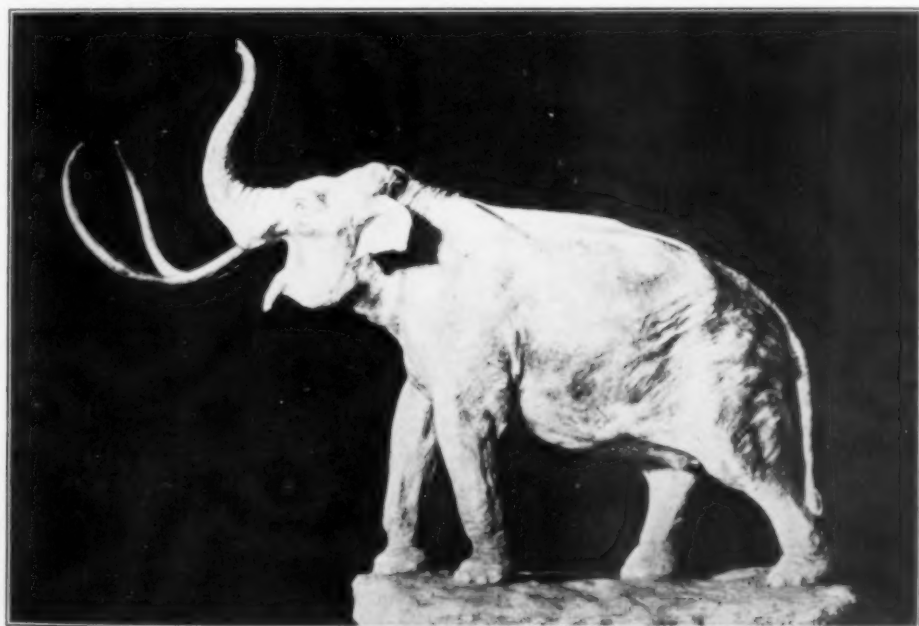
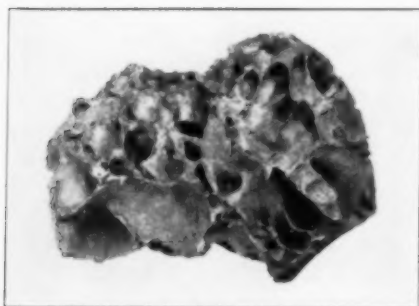


FIG. 5. A RESTORATION OF THE LARGE MAMMOTH  
ITS PROBABLE APPEARANCE AS VISIONED BY HENRY FAIRFIELD OSBORN.



—Specimen collected by John Doan

FIG. 6. THREE FRAGMENTS OF WHAT SEEM TO BE PORTIONS OF THE OCCIPITAL REGION OF A SMALL MASTODON, FOUND WITHIN THE TOWN OF YUMA, ON A HILL FULLY 200 FEET ABOVE THE COLORADO RIVER, IN LOOSE GRAVEL. THE SMOOTH AREA TO THE LEFT IS A PART OF THE BRAIN CASE. THE SURROUNDING CAVITIES ARE DIPLOIC AIR SPACES, FOR PROTECTION OF THE BRAIN AND TO LIGHTEN THE HUGE CRANIUM.

Pictographs (Fig. 9) furnished subjects for discussion, but none of us knew what the figures meant. They seem to have been pecked into the irregular surfaces of an abrupt but rapidly disintegrating basalt cliff. Fragments of rock, bearing pictures or parts of pictures, are falling from the cliff, and in a few years these interesting relics will be gone.

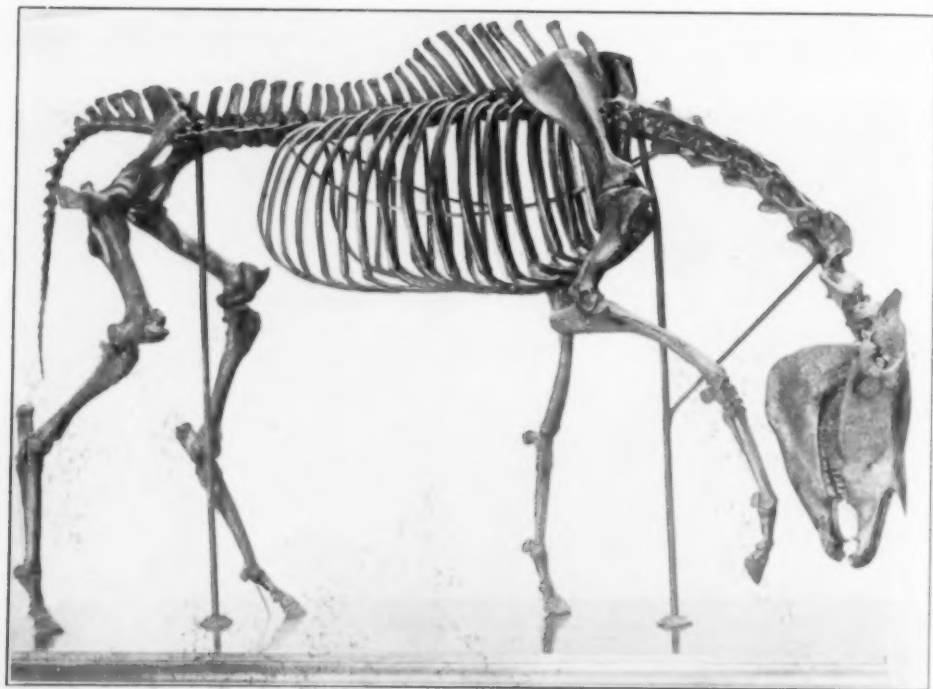
The ancient life of Arizona is not well known, and I have been actuated by a desire to increase the records by giving this brief account of what I have seen.

The following list of publications relates, in part, the scientific sources of information concerning ancient times in Arizona:

Bryan, Kirk

1925. The Papago Country, Arizona. U. S. Geological Survey Water-Supply Paper, No. 499.

On pages 68-69 are given J. W. Gidley's (Smithsonian Institution) report on the finding



—Courtesy of Los Angeles Museum

FIG. 7. SKELETONS OF THE *EQUUS OCCIDENTALIS* ARE FOUND IN THE RANCHO LA BREA BEDS, PLEISTOCENE OF LOS ANGELES.

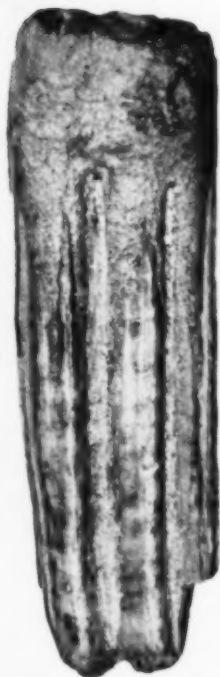


FIG. 8. A SINGLE LOWER MOLAR OF AN EQUINE, FOUND NEAR YUMA, MAY REPRESENT THE WESTERN HORSE, *Equus occidentalis*. GIDLEY HAS IDENTIFIED FOSSILS FROM THE AREA AS OF THIS SPECIES.

of the Pleistocene horse—*Equus occidentalis*), the fossil deer (*Odocoileus*), in Yuma County. Farrington, O. C.

1899. A Fossil Egg from South Dakota. Field Museum Publication, 35. Discusses and figures a fossil egg, unlike the one described by Morgan from Arizona.

Loomis, F. B.

1926. The Evolution of the Horse. Pp. 1-233, 25 plates, 41 figures.

Mallery, Garrick

1893. Picture-Writing of the American Indians.

Tenth Ann'l Report Bureau American Ethnology, 1888-89. Pp. 1-822. 1290 figures. Arizona, pp. 48-51, figures 5-9.

Morgan, Wm. C., and Tallmon, M. C.

A Fossil Egg from Arizona.

Univ. Cal. Publ. Bull. Dept. Geology, Ill.: 403-410, 2 plates.

Osborn, H. F.

1925. The Elephants and Mastodons arrive in America.

Natural History, xxv: Arizona, p. 15.

Ross, C. P.

1923. The lower Gila region, Arizona, a geographic, geologic and hydrologic reconnaissance with a guide to desert watering places.

U. S. Geological Survey, Water-Supply Papers 498 i-xiv, 1-137, 233 plates. Stock, Chester

1930. Rancho la Brea: A Record of Pleistocene Life in California.

Los Angeles Museum, publ. 1, horse, p. 85; fig. 20.



—Photo by Allison Ketcherside

FIG. 9. INDIAN PICTOGRAPHS PECKED ON A BASALT CLIFF, NORTH OF THE GILA RIVER BUT NEAR YUMA, ARE STILL CLEAR, AND AS MEANINGLESS NOW AS THEY EVER WERE.

## SOME ASPECTS OF THE CHEMISTRY OF GREEN LEAF CELLS<sup>1</sup>

By Dr. HUBERT BRADFORD VICKERY

THE CONNECTICUT AGRICULTURAL EXPERIMENT STATION

IN the summer of 1853 two young men, a German of twenty-seven and an American of twenty-three, met in Erdmann's laboratory in Leipzig. The elder was Erdmann's laboratory assistant; the younger was a student who was spending a few months learning the methods of chemical analysis before proceeding to Munich to study under Liebig. Although it is not at all obvious that the meeting of two young men seventy-seven years ago in a German laboratory should have any particular significance, it is one of my objects to show that the ultimate effects of this event are of considerable importance to-day. One of the young men was Heinrich Ritthausen; the other was Samuel William Johnson.

Johnson had long entertained the view that the application of science was essential to the further development of agriculture. His experience in Germany confirmed this idea, and the development of the system of agricultural experiment stations in this country is largely traceable to his efforts. These institutions were founded with the objects of promoting scientific agriculture, of protecting the public from fraud and of fostering fundamental scientific research. The first of them in America was established on a preliminary basis in Connecticut in 1875. A reorganization was made two years later, and Johnson became the director. Although the financial resources of the new institution were almost microscopic at first, all three of the objects of experiment station work were worthily fulfilled. In 1887 an Act of Congress, the Hatch Act, granted fed-

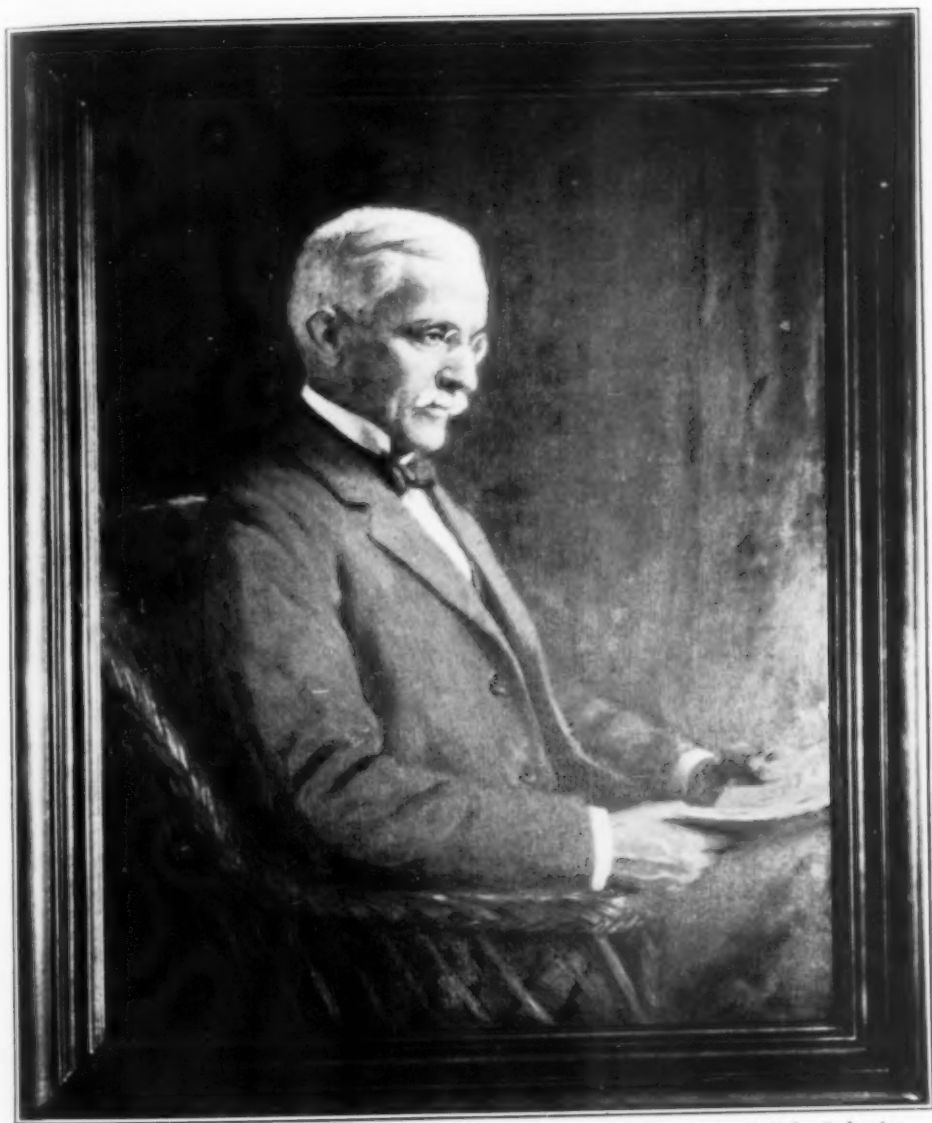
eral funds for the express purpose of supporting original scientific investigations in experiment stations. This permitted an enlargement of their activities, and it is significant that one of the first projects undertaken at the Connecticut station was an investigation of vegetable proteins.

In order to account for this let us consider for a moment the career of the other young man of the German laboratory. Ritthausen in 1854 became director of the agricultural experiment station at Möckern, near Leipzig, where work was being conducted of sufficient interest to give rise to several articles from S. W. Johnson's pen in the *Country Gentleman* for 1854. Ritthausen later worked at Waldau and at Poppelsdorf and, finally (1873), was appointed professor of chemistry in the university at Königsberg, where he remained until his retirement in 1899. In 1872 he published a slim volume<sup>2</sup> of some 250 pages in which was described an astonishing amount of original investigation on the proteins of vegetable seeds.

These investigations were little appreciated at the time of their appearance either in Germany or in this country; but among the few men who realized their value was S. W. Johnson, in New Haven. At that time little could be done, but the accession of funds at the Connecticut Experiment Station, because of the passage of the Hatch Act, made it possible to begin investigations along the lines pointed out by Ritthausen. Accordingly, in 1888, Johnson

<sup>2</sup> H. Ritthausen, "Die Eiweisskörper der Getreidearten, Hülsenfrüchte und Ölsamen," Bonn, 1872.

<sup>1</sup> A lecture delivered at the Carnegie Institution of Washington, D. C., April 30, 1930.



—From a portrait by Sodersten

SAMUEL WILLIAM JOHNSON

suggested to his assistant and son-in-law, Thomas B. Osborne, that a study of the nitrogenous substances in plant seeds should be undertaken. The report of the director for 1889 carries the terse statement: "Much time has been given by Dr. Osborne to a study of the nitrogenous matters contained in the kernels of maize and oats." In the following year the first of Osborne's papers on the proteins of vegetable seeds appeared, the beginning of a series of papers on proteins and allied subjects that extended in unbroken sequence for thirty-eight years. The interest that Ritthausen had aroused in Johnson's mind during the student years in Europe thus bore extraordinary fruit.

It is not my present purpose to discuss Osborne's long-continued investigations of the vegetable proteins, important and significant as these may be. I may draw attention, however, to one interesting point. As was the case with Ritthausen in Germany, little attention was at first paid to Osborne's work even by his chemist friends; but the compliment that Johnson had paid to Ritthausen was gracefully returned by Germany when Griessmayer, in 1897, translated all Osborne's papers that had appeared up to that time and published them in book<sup>3</sup> form with the object, as he stated in his preface, of "bringing to light these treasures hidden in the American journals."

This spiritual encouragement was followed in 1904 by encouragement of a more substantial nature when the Carnegie Institution of Washington made its initial grant of funds to support Osborne's investigations. Since that date those who are accustomed to look through the year book of the institution have found each year a few pages in which the progress of his studies of the vegetable proteins has been briefly reported. Taken together these reports

describe one of the most extraordinary accomplishments of American biochemistry. It is work upon which the very modern science of nutrition is securely founded.

It is my purpose to discuss in detail one phase only of Osborne's investigations; it is a phase in which he was especially interested during the last years of his life, and it led to the studies upon which the laboratory he organized is now engaged.

The development of the vitamin hypothesis about fifteen years ago, in which Osborne and Mendel, with the support of the Carnegie Institution, were largely instrumental, drew the attention of investigators to the enormous importance of "little things" in nutrition. When animals were restricted to a diet composed of purified fat, carbohydrate, protein and inorganic salts, nutritional failure and serious organic disturbances resulted, unless small addenda consisting of cell material or cell extracts were supplied. Such knowledge as was available showed that the active principles in these addenda were present in only minute amounts. These observations focused attention upon the extremely limited nature of the available information on the chemical composition of cells and emphasized the necessity of thorough investigation of them. A broad field of research was thereby revealed. The accomplishments of the past ten years show that this type of work is extraordinarily fruitful. It is necessary only to refer to Sir F. Gowland Hopkins' discovery of glutathione in yeast and to the results of the intensive investigation of the tuberculosis bacillus now being carried on in this country to illustrate my meaning. Osborne and Mendel's feeding experiments had shown that green leaf cells were valuable sources of those highly potent nutritional principles, the vitamins. This observation was the more interesting since green leaves form almost the entire food of many animal species;

<sup>3</sup> V. Griessmayer, "Die Proteide der Getreidearten, Hülsenfrüchte und Ölsamen, sowie einiger Steinfrüchte," Heidelberg, 1897.



THOMAS B. OSBORNE

it would therefore appear that in them nature had provided an adequate diet.

Leaves have long been known to contain proteins. The precipitate that separates when leaf juices are heated was recognized a hundred and forty years ago to be very like the coagulum produced by heating egg white and was designated "albumin," a name that recalls this similarity. The Germans were even more specific with their designation "Eiweiss."

Although many attempts have been made to ascertain how much protein leaves contain, the problem is one that is still unsolved. The leaf proteins are enclosed within cell walls which must be ruptured before all the protein can be extracted. This is extremely difficult to do, thoroughly, on a sufficiently large amount of material. The cell contains enzymes that rapidly convert one constituent into another. The activity of these enzymes can not be suspended during the operations of grinding and extracting the protein; accordingly one can scarcely hope to find unaltered cell protein in such extracts. Furthermore, the cell contains a large number of non-protein constituents, of known and of unknown nature, from which the protein must be separated in order to obtain preparations that even roughly approximate purity. In short, in the leaf one is faced with a condition of physiological activity totally different from that encountered in the seed. The proteins of seeds are reserve stores of food that are not called upon until growth of the embryo takes place. As laid down in the seed they have considerable chemical stability and, consequently, can be extracted and isolated with some ease in relatively pure form. But the leaf protein is one of the reagents in the complex series of chemical events that is recognized as life. What part it plays in metabolism is unknown, but certainly it has something to do with the delicate

chemical and physicochemical equilibria within the cell.

These considerations indicate that attempts to isolate the proteins of the green leaf are, from one point of view, the pursuit of a will-o'-the-wisp. It is highly improbable that the substance ultimately secured and, with a feeling of triumph, labeled green leaf protein is identical with the stuff that shared in the reactions of the cell. It is even less certain that the preparations represent homogeneous chemical entities. On the other hand, from such preparations much can be learned of the complex nature of cell reactions and much also of the value of green leaves as animal food.

Consequently, in spite of the many uncertainties involved, Osborne<sup>4</sup> attempted about ten years ago to isolate the proteins of spinach and alfalfa leaves. He recognized the desirability of discovering as much as possible about the composition of some single, but typical, cellular material, in the hope that the information might lead to generalizations upon the subject of cell composition. He realized, however, that worth-while results could be secured only after suitable methods for such investigations had been developed, and, as the work has gone on, it has become increasingly clear that the crux of the whole problem is this question of methods.

Large quantities of leaves were thoroughly disintegrated by grinding repeatedly in a suitable mill. The pulp was then subjected to pressure in a hydraulic press. In this way a clear fluid, the undiluted juice of the cell, could be obtained. When this was treated with alcohol, a precipitate separated that was mainly a mixture of proteins and inorganic salts. This precipitate was treated in various ways with the object of re-

<sup>4</sup> T. B. Osborne and A. J. Wakeman, *J. Biol. Chem.*, 42: 1, 1920; T. B. Osborne, A. J. Wakeman and C. S. Leavenworth, *J. Biol. Chem.*, 49: 63, 1921.

moving the non-protein impurities. The most effective method was to warm the material for a short time at 80° in the presence of a weak solution of alkali in diluted alcohol. Under these conditions the substance largely dissolved, and the clear filtrate, when neutralized, yielded a flocculent precipitate that contained 16.36 per cent. of nitrogen and gave all the reactions of proteins. The treatment at 80° with dilute alkali profoundly altered the properties of the crude protein. Evidence was obtained that a non-protein group was thereby split off, and it appeared probable, therefore, that a considerable part of the crude protein material was of the so-called conjugated type. The conjugate group was not identified but was a colored substance of complex nature.

The leaf residue from which the clear juice had been expressed retained about one half the nitrogen of the original leaf. Surprisingly little of this nitrogen could be extracted by dilute aqueous alkali, although this reagent is usually a very effective solvent for proteins. The residue was therefore treated with dilute alcoholic alkali for a few minutes at boiling temperature. This reagent extracted nearly all the residual nitrogen and preparations of protein could readily be secured from the solutions.

The table shows the quantitative results thus secured. It is evident, from

the cells of the leaf escaped rupture during the grinding and pressing operations and the extractions. Examination of the different extracts indicated that only from 66 to 75 per cent. of the leaf nitrogen belonged to proteins, but so rough an estimate as this was not at all satisfactory. Furthermore it was impossible to determine from the data what parts of the cell were represented by the different fractions.

The failure of these methods to give results that could be interpreted in terms of cell physiology induced A. C. Chibnall, who worked with us from 1922 to 1924, to devise a method<sup>2</sup> whereby the soluble contents of the cell vacuole might be removed before the cell walls were ruptured by grinding. It has long been known that a fluid can be readily expressed from leaves after plasmolysis of the cells by freezing or by the vapors of ether or chloroform. Exactly what happens under these conditions is a matter of debate. When a leaf cell is treated with ether while under microscopic observation the cytoplasm that lines the cell wall can be seen to shrink together and the vacuole liquid within the cytoplasm to pass through it. The permeability of the cell is entirely changed, and the forces that retained the structures *in situ* no longer operate. Chibnall found that, if leaves of spinach or alfalfa are immersed in ether, plasmolysis occurs very promptly and the whole fluid contents of the cells can then be readily expressed under the hydraulic press. The nearly dry leaf residue rapidly imbibes water and, by alternately adding water to the loosened presscake and pressing, a thorough washing can be effected. By this method the cell walls are not ruptured and the formed elements within the cell are retained.

The vacuole fluid, when heated to 85°, gave a small coagulum of protein. The

<sup>2</sup> A. C. Chibnall, *J. Biol. Chem.*, 55: 333, 1923.

	SOLIDS		NITROGEN	
	gm	Per cent.	gm	Per cent.
In alfalfa taken	4,442	100	211.6	100
Press juice	1,898	42.7	92.6	43.8
Alcohol extract	283	6.4	4.3	2.0
0.3 per cent. aqueous NaOH	230	5.2	14.8	7.0
0.3 per cent. alcoholic NaOH	761	17.8	83.2	39.3
Extracted residue	1,292	29.1	11.1	5.3
Total	4,464	101.2	206.0	97.4

the small proportion of the total nitrogen in the final residue, that very few of

amount in the case of alfalfa was very small and amounted only to the equivalent of 0.25 per cent. of the leaf solids and 0.56 per cent. of the leaf nitrogen. It was clear that very little if any of the leaf protein occurs dissolved in the vacuole fluid. The bulk of the protein must therefore be present in the jelly-like cytoplasm that had not passed through the cell walls. The presscakes were thoroughly ground with the addition of a liberal amount of water; the coarser particles were strained out on fine silk, and the turbid aqueous suspension was filtered on a thick pad of paper pulp. A clear yellow-brown solution was secured which, when treated with a very small amount of dilute acid, yielded a copious precipitate of protein. The precipitate was removed, dissolved in a little alkali, reprecipitated by neutralization and then extensively washed by alcohol and ether which removed small amounts of impurities. It contained 16.25 per cent. of nitrogen and gave all the reactions of proteins.

This preparation represented a much more clearly defined entity than did the material secured from alfalfa juice by alcohol precipitation. It is that part of the cytoplasmic protein that passed into solution during the grinding operations. There were grounds for supposing, therefore, that it represented a part, at least, of the protein of the cytoplasm in a not extensively altered condition. Time is an important factor in success with this type of work. If the leaf residue is allowed to stand a few hours before working up, the yield is greatly reduced, while if twenty-four hours elapse only negligible amounts of protein can be obtained. The protein actually secured under the best conditions represents little more than a fifth of the probable amount in the tissue. The great reduction in yield brought about by delay in the grinding operations renders it likely that rapid *post-mortem* changes in

the solubility of the cytoplasmic protein occur, as a result of which it becomes increasingly difficult to disperse the substance into colloidal solution by grinding with water.

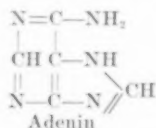
The results of the work on the proteins of green leaves have been, on the whole, decidedly disappointing. The difficulties of preparing representative samples of them are almost insuperable by any method that has yet been devised and, as I suggested a few moments ago, there is little evidence that the material isolated and labeled is indeed the substance that plays so vital a part in the activities of the cell. But there are few problems of leaf cell chemistry more important than the investigation of these substances.

Such evidence as we have been able to secure indicates that physiologically active leaf proteins are distinctly different in their solubility relations from the better known seed proteins. Their amino acid composition, however, so far as it has been ascertained, presents nothing distinctive. All the amino acids that are known to be essential in nutrition are present in liberal amounts, and the high nutritive value of these proteins therefore receives some rational explanation.

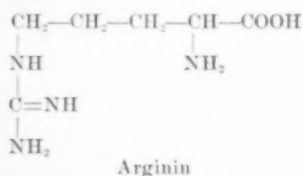
I pointed out a little while ago that these investigations had their origin in part from the observation that green leaf material displays a very marked vitamin potency. Numerous experiments with extracts of alfalfa leaves showed that the vitamin B potency appeared in the water-soluble non-protein fraction, that is, in the solution which represents the vacuole content of the cells. This observation furnished a stimulus for the examination of the leaf cell extract and gave rise to the investigations that are now being actively continued.

The investigation of green leaf extracts has been a matter of concern in some quarters for many years. The chief figure in the older literature is Schulze, the discoverer of arginin and

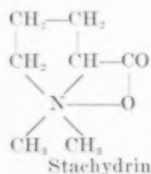
phenylalanin. A perusal of his voluminous papers shows that he was chiefly interested in the distribution of single substances in nature rather than in the detailed investigation of a single type of cell. Nevertheless, from Schulze's papers and from those of later investigators, a fairly clear idea may be obtained of the nature of the substances that are likely to be found in a given leaf extract. In general, nitrogenous bases of four distinct types are present, which are roughly classified, for practical purposes, according to the differences in their behavior towards silver salts. The purin bases, such as adenin, form silver compounds that are insoluble in acids;



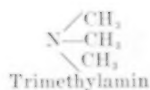
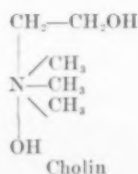
the arginin type of base forms silver compounds that are precipitated only



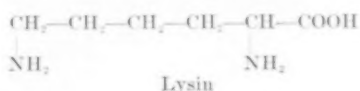
when the solution is made alkaline; the quaternary bases, such as stachydrin,



cholin and the amines, do not form in-

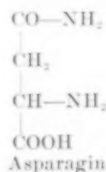


soluble silver compounds. The well-known protein hydrolysis product lysin,



which is also found in this class, exemplifies the fourth type of base.

In addition to nitrogenous bases, a complex mixture of amino acids and peptides is found in leaf extracts, together with very considerable amounts of amides of which asparagin, the amide

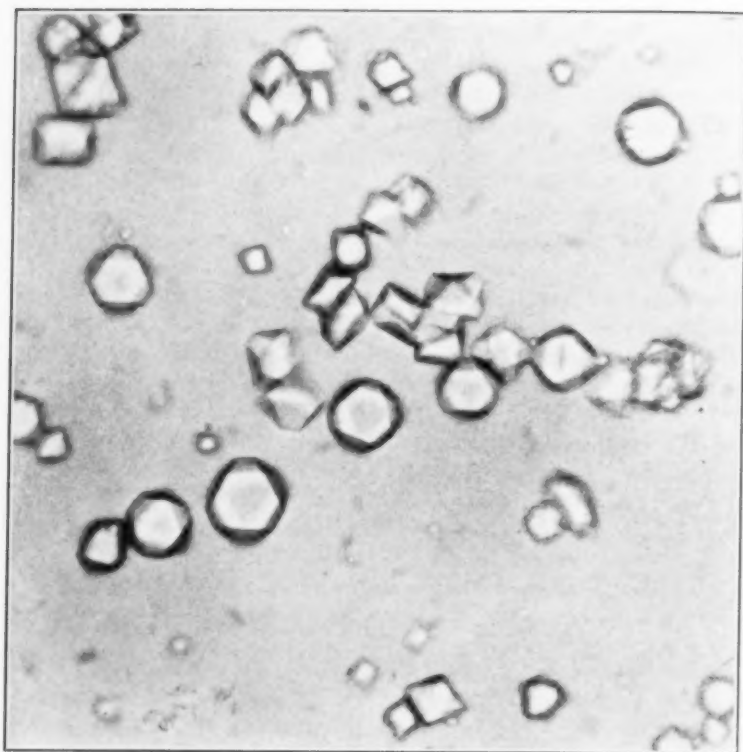


of aspartic acid, is usually the most plentiful.

Although a mixture of these nitrogenous substances only is complex enough to tax the resourcefulness of any analyst, the complexities do not end here. In addition in plant extracts there is invariably a wide assortment of carbohydrates, pectins, gums, resins and allied substances, organic acids of the malic acid



type, salts of phosphoric acid and nitric acid, of calcium, magnesium, iron, sodium, potassium and other inorganic ions and last, but most prominent and annoying of all, a mixture of pigmented substances, or the precursors of such, of an unknown nature but of astonishingly varied chemical properties.



GLOBULIN FROM TOBACCO SEED  
FIRST CRYSTALLIZED IN MARCH, 1930.

The problem we set ourselves was to isolate, in definite crystalline form, as many of these substances as possible, knowing full well that this brief list takes no account of what is unknown about the composition of the extract.

The problem quickly reduced itself to a study of methods of fractionation into main groups, methods for the analysis of these groups into their components and methods for the identification of the components themselves. This meant, in turn, the search for reagents that would precipitate definite groups of substances, so that our analytical operations could be conducted on simplified mixtures.

The essential requirements of a reagent to be employed in this kind of work are that it must precipitate a definite type of compound cleanly and quantitatively, it must precipitate as little of any

other type as possible, it must introduce no nitrogen and it must be removable from the solution afterwards in some simple manner. These requirements limit one's choice to salts of metals, such as mercury, silver, copper and lead, with such easily removed acid radicals as the chloride, sulphate or acetate. Phosphotungstic acid and complex iodides are widely employed for certain separations. Nitrates, phosphates and alkali ions such as sodium or potassium must be avoided as much as possible.

It is clear, then, that our choice is severely circumscribed by considerations of a purely practical nature. We have nevertheless been able to select a sequence of reagents to be used as precipitants, and the scheme we finally adopted is relatively simple in outline. It differs fundamentally from any

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scheme previously employed for this purpose, but, as our investigations proceed, we are becoming increasingly confident in its power.

The first step is the preparation of an extract of the leaf material that shall contain as much of the simpler substances of the cell sap as possible. The grinding and pressing operations employed by Osborne in his earlier work on leaf proteins yield a fluid that is essentially the undiluted vacuole content together with much protein. This fluid was freed from protein by treatment with an equal volume of alcohol, and the filtrate from this precipitate was employed in all our work on alfalfa.<sup>6</sup> Recently, however, we have prepared extracts by the much simpler method of plunging the fresh leaves into boiling water and then cooking them for a short time. In this way the proteins are coagulated, the enzymes are destroyed immediately and, by pressing the leaves and washing the presscakes thoroughly with hot water, we secure extracts that contain practically the whole of the simpler nitrogenous substances. Doubtless chemical changes of an irreversible nature occur during these operations but, for the present, we regard the losses as being far outweighed by the gains. This applies particularly to the inactivation of the enzymes. We were always apprehensive, when dealing with leaf extracts made by the older method, that extensive changes might have occurred before the enzymes in the extracts were finally destroyed by heating.

The extract is concentrated and treated with one to two volumes of alcohol to remove residual protein and much of the inorganic salt. The filtrate is then freed from alcohol.

Barium hydroxide must be used repeatedly during the analytical operations on the cell extract, and this base forms insoluble compounds with many of the organic acids and other substances

that are present. In order to remove these we therefore next add an excess of this reagent and filter off the precipitate. We have found barium hydroxide superior to basic lead acetate, the preliminary reagent employed by Schulze and almost all others who have undertaken this type of investigation, since little nitrogen is precipitated by it and the precipitate forms admirable material for the study of the organic acids of the leaf.

The next step is the precipitation of the organic bases and amino acids. Neuberg and Kerb<sup>7</sup> showed many years ago that  $\alpha$  amino acids and many bases could be quantitatively precipitated by mercuric acetate from a solution that was maintained faintly alkaline with sodium carbonate. So far as we are aware no other reagent does this so effectively. Although the procedure involves the use of sodium carbonate and therefore violates one of the primary principles we have laid down for the selection of reagents, we here have a situation where the gains outweigh the losses.

The reaction that occurs is not without interest. The compound that is formed, when mercuric acetate and sodium carbonate are added to a solution of an amino acid, contains carbon dioxide, and is analogous to the carbamino acids employed long ago by Siegfried in his investigations of amino acids, and more recently by Buston and Schryver.<sup>8</sup> It is apparently a basic mercuric salt of the carbamino acid derived from the amino acid.

When mercuric acetate and sodium carbonate are added alternately to the leaf extract a copious precipitate is produced that becomes yellow in color when excess of both reagents is present. An equal volume of alcohol is finally added to promote the flocculation of the precipitate, and this is then removed, washed and suspended in water, acid-

<sup>7</sup> C. Neuberg and J. Kerb, *Biochem. Z.*, 40: 498, 1912.

<sup>8</sup> H. W. Buston and S. B. Schryver, *Biochem. J.*, 15: 636, 1921.

<sup>6</sup> H. B. Vickery, *J. Biol. Chem.*, 65: 81, 1925; 65: 657, 1925.

fied with sulphuric acid and decomposed with hydrogen sulphide.

The precipitate contains all but insignificant amounts of the amino nitrogen of the extract and all the organic bases except the quaternary bases and amines. The filtrate is acidified with hydrochloric acid and freed from mercury. It contains the quaternary bases and amines, the sugars, insignificant residues of amino acid material due to the solubility of the mercury precipitate and a large amount of sodium chloride.

By this fractionation we greatly simplify our analytical problem, since in the mercuric acetate precipitate we have substances that are for the most part allied to the protein decomposition products for which methods of analysis have been devised; in the filtrate we have basic substances of a fairly uniform type for which also analytical methods are in existence.

Theoretically, then, the problem, while complex enough, should be capable of solution. In practice, however, we encountered many difficulties, and these were of such an acute nature that for the past three or four years we have spent nearly all our time on the development of the existing methods of protein analysis with the view of rendering them more readily applicable to the project in hand.

It may be of interest to point out the nature of some of these difficulties. For many years it has been customary, when one must separate a mixture of mono-amino acids and basic amino acids, to employ phosphotungstic acid to precipitate the basic substances. The precipitate can be readily decomposed by barium hydroxide and the bases quantitatively recovered. The excess of reagent can be equally readily removed from the filtrate which should then contain nothing but the mono-amino acids. This procedure was therefore used in our work on the extract from alfalfa. When the voluminous precipitate was decomposed in the usual way we were some-

what disconcerted to find that a considerable part of the nitrogen in it could not be recovered from the insoluble barium phosphotungstate. Evidently phosphotungstic acid must be used to precipitate these bases only if no better method could be devised.

Another source of difficulty was the highly pigmented substances in the extract. Some of this annoying material found its way into nearly every fraction, and we seldom had the comfort of working with colorless solutions.

The most serious difficulty of all was, however, a feeling of uncertainty regarding the details of the methods of analysis that were available. The great experience of one or two of the workers on our staff with the analysis of proteins was indeed a material help, but we felt that a detailed study must be made of many points before we could attack the analysis of plant extracts with confidence.

The results of an analysis of the simpler nitrogenous constituents of an extract of alfalfa leaves may be mentioned as an illustration of the inadequacy of these methods. In spite of the greatest care we were able to account for only approximately 30 per cent. of the solids and 55 per cent. of the nitrogen of this extract in the form of well-characterized products weighed in crystalline form. While it is probable that a considerable part of the undetermined nitrogen belonged to some of these same substances that we were unable to bring to crystallization in a form fit for weighing, it is clear that much of the nitrogenous material in this extract is of as yet unknown nature.

One very important conclusion became clear from this investigation. In recent years there has been a tendency to apply indirect methods of analysis to plant ex-

tracts. These methods are founded on the behavior of certain substances with specified reagents. For example, when arginin is heated with strong alkali, one half its nitrogen is split off as ammonia, and from the amount of ammonia so formed one can calculate the amount of arginin present with considerable accuracy. This method works fairly well in the relatively simple mixture of bases derived from the hydrolysis of proteins. When it is applied to a plant extract the assumption is made that no other substance is present that is decomposed by alkali to liberate ammonia, an assumption that, in our opinion, is hardly justified. As a result of our experience, we feel that few indirect methods of this type can be safely applied to these complex and largely unknown mixtures. The only adequate criterion for the presence of a substance is a crop of crystals of the substance itself, or a derivative of it, that can be subjected to detailed analysis and proper identification. Indirect methods have their place and are frequently of immense value, but they can be applied only when a demonstration is provided that there is no interference from other constituents of the mixture.

Our general dissatisfaction with the details of base analysis, as usually employed, led us to an extensive investigation of these methods. In the hope that a higher degree of precision might be attained when simple mixtures were taken for analysis we turned to proteins, since these when hydrolyzed yield, so far as is known, only three basic amino acids, histidin, arginin and lysin. It further happens that these three are, with respect to their behavior towards silver salts, typical representatives of three of the four main classes of organic bases already mentioned.

I may pass over the details of these investigations, since they have little interest to those not engaged in this type of work; the net result has been that we

have brought the methods for the quantitative analysis of the basic amino acids yielded by proteins to a point where a very high degree of precision can be attained. This work has an intrinsic value aside from its application to the analysis of plant extracts. The physical chemists now require highly accurate determinations of the basic and of the acidic amino acids derived from proteins for the further development of the theory of proteins. We believe that our methods provide the requisite degree of accuracy as regards the basic amino acids, and the work of Dr. D. B. Jones,<sup>9</sup> of the protein and nutrition division of the Bureau of Chemistry and Soils, has recently supplied modifications of the methods for dealing with the acidic amino acids which are capable of yielding highly accurate results.

Although our study of the methods of protein analysis has not yet been brought to a conclusion, it had reached a point some time ago where it seemed desirable to return to the subject of leaf cell analysis. We wished to select a material that would fulfil a number of requirements that our previous work had shown to be of importance. The alfalfa leaf is very small, and as our earlier work was done upon the plant as cut with a scythe, leaves and stalks were both included in the material examined. For the new work a large leaf was desirable. The extract was to be made by plunging the leaf into boiling water so as to stop enzyme action and kill the cells immediately; a thin leaf was therefore needed. Furthermore, we desired to have leaves from plants all of one fixed variety, of the same age, grown under the same conditions of nutriment and reproducible from year to year.

No plant appeared to fulfil these strict requirements so well as the tobacco plant. We have in Connecticut a branch of the experiment station devoted to the study

<sup>9</sup> D. B. Jones and O. Moeller, *J. Biol. Chem.*, 79: 429, 1928.

of tobacco, and this branch is in a position to supply material of a highly constant type grown year after year under precisely similar conditions and under expert supervision. Furthermore, in view of the economic importance of the tobacco crop in the state, the station looked with great favor on our suggestion and offered the most generous assistance.

We have therefore been engaged for some time in a preliminary study of tobacco. This study has, to the present, been devoted almost wholly to the solution of the special problems that arise from the presence of the volatile alkaloid nicotine, and to devising methods for the determination of different forms of nitrogen in the presence of the nitrates that are usually found in considerable amount. I shall not weary you with a detailed report of these investigations. I wish only to draw attention to two points since they illustrate our method of approach.

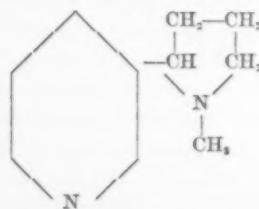
Although it is widely believed that the tobacco plant normally contains a considerable proportion of nitrate, an examination of the literature revealed that the evidence for this is based, from the qualitative side, upon color reactions, while the quantitative estimations of the nitrate have always been carried out by indirect methods that were not entirely above criticism.

We wished first to prove that the substance in the leaf that has been regarded as nitrate was in fact this substance. A specimen of tobacco was therefore treated with an excess of calcium hydroxide, was then dried and extracted with alcohol, whereby we hoped to extract all the nitrate as calcium salt. The alcohol extract was evaporated, the residue was dissolved in water, filtered, and was then treated with nitron, an organic base that forms a very insoluble crystalline compound with nitric acid. Pure crystalline nitron nitrate was readily secured, and the quantity found corresponded to over 90 per cent. of the

nitric acid that the indirect quantitative methods had indicated was present in our original specimen of tobacco. This made it certain that we were really dealing with nitrates in this plant.<sup>10</sup>

The tobacco plant contains very considerable amounts of nicotine, and this substance is distributed throughout the plant, in leaf, stem, root and flower. As the ovaries in the flower develop, the proportion of nicotine in the ripening seed diminishes, and the fully matured seed of the plant is, so far as we have been able to detect, entirely free from nicotine. When matured seed is scattered on blotting paper, moistened with distilled water and kept at the right temperature and humidity for about ten days, a sprout from 2 to 3 cm long develops. The sprouts can be removed from the seed and an extract of them prepared. Such an extract was found, by the customary indirect method of analysis, to contain nicotine in an amount equivalent to 0.3 per cent. of the dry weight of the material. As this was a matter of too great significance to admit of uncertainty we prepared a considerable quantity of the sprouts, isolated the nicotine from them and converted it to the beautifully crystalline dipicrate which was identified beyond possibility of doubt.

Inasmuch as the sprouts grew entirely upon the stores of food laid up within the seed, it is clear that the nicotine we prepared from these sprouts arose from precursors within the unsprouted seed; no outside source of nutriment was available to the embryonic plant. Now nicotine possesses a highly characteristic mo-



Nicotine

<sup>10</sup> H. B. Vickery and G. W. Pucher, *Ind. and Eng. Chem., Analytical Edition*, 1: 131, 1929.

lecular configuration. It is formally de-



rived from the extremely stable nitrogenous substance pyridin that was first encountered in the so-called Dippel's oil, an oil prepared by the distillation of bones, and is also found among the products of the distillation of certain kinds of coal. It seems reasonable that among the substances in the seed of the tobacco plant there must be one that contains this interesting ring structure.

In the search for this precursor of nicotin our thoughts naturally first turned to the proteins of the tobacco seed. Although there is no known constituent of proteins that contains the ring structure of pyridin, it is by no means beyond the bounds of possibility that such a structure may occur in some proteins, particularly the proteins of the tobacco seed. We have therefore, as it were, turned our laboratory clock back thirty years or more and are again studying the proteins of a seed according to Osborne's classical methods. We found that the tobacco seed contains a globulin that could be readily obtained in a beautifully crystalline form. Inasmuch as not many more than a dozen vegetable proteins have hitherto been described in the crystalline condition, and these for the most part by Osborne himself, this result was most gratifying. Our studies of the newly isolated protein are now being carried on. Whether or not it contains the desired precursor of nicotin, the investigation of so well-characterized a protein is itself a matter of interest.

I have endeavored to present a brief story of the activities of the laboratory at New Haven. In considering these matters while preparing this discussion,

I have been struck by the painful slowness with which results of a worth-while nature are secured. In a narrative stripped of discussion and explanation it would be relatively easy to tell you in ten minutes of all that we have actually accomplished in a period of ten years. Such a narrative would, of course, leave out of consideration the innumerable false starts and actual failures, and the indescribable feelings with which one watches a month's hard work drip out of a cracked beaker and run to waste across the laboratory table. It would leave out, also, the hours of study and concentrated thought that precede the birth of an idea. But perhaps most important of all, it would leave out entirely the joy that one takes in the rounding out of a new hypothesis, the crystallization of a long-sought substance or the discovery of a new fact.

Scientific research is like a journey across a broad country. One travels now up, now down, and sometimes one descends very deeply indeed into interminable valleys; but occasionally one reaches an eminence from which a wide view of the whole can be obtained. Such is the reward for effort.

In conclusion may I quote the last few sentences from the last paper prepared by Dr. Osborne for publication.<sup>11</sup>

It must not be forgotten . . . that the real chemistry of cells is far too complicated to be dealt with adequately by a study of the constituents of the mixture in the fluid obtained from them. Microscopic examination of the original cells shows them to be complex structures containing chloroplasts, nuclei, vacuoles, etc., each of which parts implies a different chemical structure. I fear that for a long time to come much will still remain to be learned about the chemistry of the cell, but if, in the meantime, we can extend our knowledge of this subject it may save us from many erroneous conclusions based on incorrect results that were obtained without sufficient appreciation of the real nature and complexity of the problem.

<sup>11</sup> T. B. Osborne, *Leopoldina*, Ber. kais. Leopold. deut. Akad. Naturforscher Halle, 4: 228, 1929.

# THE ULTRASCOPIC VIRUSES FROM THE BIOLOGICAL STANDPOINT

By Professor E. W. SCHULTZ

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THOUGH most people have never experienced the thrill of a microscopic visit to that vast world of miniature plants and animals which surrounds us everywhere, few nowadays question the existence of microbic life. The term "germ" has become a household word. In the evolution of living forms these simpler precursors of higher living forms are probably to be expected. To the biologist they represent the simplest units of life which he can study under his microscope. Indeed, the average biologist does not anticipate much of interest to him below the range of his instrument. This is easy to understand if we bear in mind that with a good microscope one can still discern by direct illumination bodies as small as 0.25 of a micron (or 1/100,000 of an inch) in diameter and that these just discernible living bodies present little for structural analysis. Indeed, with optical facilities which enable one to see and study cells of this minute size, a sort of doubt has grown up that anything much of importance to a biologist could lie outside the range of his ordinary optical devices. Assuredly, how could there be much when the smallest cells still within range of his instruments present so little actually to distinguish them from non-living spheroids, presenting as they do no discernible nucleus or other distinctive structural features, only the visible evidence of their proliferation and of their chemical activities bearing witness to their living nature?

While certain good reasons have tended to support the supposition that nothing of significance could lie below the range of microscopic vision, the dis-

covery by Iwanowski thirty-seven years ago that the virus of tobacco mosaic disease, a transmissible disease attended by a peculiar mottling and dwarfing of the affected plants, is capable of passing the pores of a fine porcelain filter, at any rate suggested the probable existence of life much below the range of our best optical instruments. Indeed, Beijerinck, who five years later rediscovered the filtrability of this virus, actually postulated the agent as a living contagious fluid (*contagium vivium fluidum*), a deduction not so illogical if we remember that the colloidal state of matter was then not as generally known as now.

While these studies on mosaic disease of the tobacco plant served to open the door to an entirely new group of infectious agents, exemplaries of the group have come to us in largest numbers from studies on the causal agents of various animal diseases. In the year that Beijerinck in Holland rediscovered the filtrability of the virus of tobacco mosaic disease, Loeffler and Frosch in Germany, while engaged in an endeavor to separate a toxic substance for immunization purposes from the lesions of foot-and-mouth disease of cattle, discovered that the agent itself of this disease is capable of passing the pores of fine porcelain filters. Also in the same year Sanarelli in South America discovered the filtrable nature of the virus responsible for a strange malady among his stock rabbits known as infectious myxomatosis of rabbits. These interesting observations made in the course of a single year in different countries naturally prompted similar inquiries into the causation of a number of other diseases the etiology of

which had hitherto remained uncertain or entirely unknown. More than thirty such filtrable agents are now known to be responsible for infectious diseases. Among them are the agents of such familiar diseases as smallpox, cowpox and pox diseases of other animals, rabies, poliomyelitis, fowl plague, cattle plague, leukemia of chickens, sarcoma of chickens, hog cholera, distemper of dogs, encephalitis of certain fur-bearing animals, certain diseases of insects and certain diseases of fish. Recent additions to the list include the bacteriophage, the virus responsible for so-called "parrot fever" or psittacosis, and yellow fever.

As important as many of these ultrascopic viruses are from the medical and economical standpoints, it is not my purpose to enlarge on these aspects of the general problem which they present. Quite apart from the urge for practical information, which the investigator naturally can not disregard, the ultraviruses present much of interest from the standpoint of purely biological research. What are they? Are they living agents, or are they perchance non-living agents resembling only outwardly, as they spread from host to host, a truly living parasite in the transmissible effect which they produce? As living agents they would certainly challenge the interest of any biologist, for what biologist is not interested in the most primitive forms of life? Should they be inanimate in nature, interest in them from a biological standpoint would not diminish, for the attention of the biologist would then merely shift to the strange phenomenon which causes a virus-tainted cell to regenerate the selfsame agent which induced the original disease process. The biologist would then want to know what starts off such a pernicious cycle in nature, and also something regarding the precise mechanism underlying such a disturbance. It can be seen, then, that, whatever the exact nature of these agents may be, they pre-

sent considerable in the way of interest from the purely biological standpoint.

Let me sketch briefly some of the more important characteristics which tend to set the ultrascopic viruses and the diseases which they produce off in a class by themselves.

#### PROPERTIES OF ULTRAVIRUSES

##### Ultrafiltrability

To say that a virus is filtrable in the sense that it passes through the pores of an ordinary porcelain or kieselguhr candle does not tell us much as to the actual size of the agent passing the filter. On the one hand, one can not be certain that the agent passing such a filter is actually of subvisible dimension, and, on the other hand, one can not always be certain that a negative result precludes the presence of a virus. A number of factors, such as the composition of the filters, their electrical charge, the pressure used, the nature and reaction of the menstruum, etc., may greatly influence the results of any filtration experiment, particularly when earthen filter candles are used.

While filtration through ordinary filter candles tends to yield a rough separation of microscopic and submicroscopic forms, filtration through graded collodion membranes, the pore size of which may within certain limits be controlled, enables one to determine with a fair degree of approximation the physical magnitude of the agent under observation. Even under these conditions it is difficult, if not practically impossible, to make accurate measurements of the size of virus particles. This difficulty is made more real by virtue of the fact that none of these agents with the possible exception of the bacteriophage has been obtained in even a relatively pure state, free of protein and other aggregates derived from the host, which in themselves greatly disturb the results of any filtration experiment. However, despite these obstacles, some interesting and, I trust, significant measurements

have thus far been made by means of these graded collodion membranes. While the results of these measurements must be regarded as approximations only, they nevertheless tend to confirm the suspicion that in some of the so-called filtrable viruses, at any rate, we have a group of agents whose organization, if they be living, must be appreciably simpler than that of the smallest bacterium known to us, indeed so much more primitive that it is difficult to visualize constituent functional parts in such cells, if they may still be regarded as cells.

The virus on which the greatest number of measurements have been made is the bacteriophage, an agent discovered about fifteen years ago which produces a transmissible disease of bacteria, associated generally with an explosive dissolution of the bodies of the affected organisms. A mere trace of a dissolved bacterial culture containing this virus is sufficient to set up the diseased state in a new culture. Thousands of passages may be made in this manner from one dissolved culture to a fresh culture without the slightest diminution in the final concentration of the lytic agent, a fact which compels us to conclude that the agent multiplies at the expense of the bacteria it dissolves, by virtue of which it bears the earmarks of a true virus. Because of the tremendous interest which this discovery aroused, particularly with reference to the possible nature of the agent responsible for this phenomenon, many investigators have made a study of the properties and behavior of this interesting filter-passing agent. Indeed, the relatively simple conditions under which the nature of this virus could be studied have offered additional impetus to the study of virus problems in general, with the result that much of the work carried out in recent years on viruses in general can be credited in a large measure to the stimulus which has grown out of the studies

on the nature of the bacteriophage. As might have been anticipated at the outset two schools have grown up relative to the nature of this agent (this applies to other ultrascopic viruses also), one maintaining that it is of a living nature, the other that it is non-living in nature. Those who dispute its living nature base their stand, in part at least, upon the inordinately small size of the 'phage particle. Not content with the earlier approximations of 20 to 35  $\mu$ ,<sup>1</sup> more recent investigators have crowded it into progressively lower orders of magnitude. Most recent measurements, including some made in our laboratory, place it in the neighborhood of 5  $\mu$ . Expressed in another way, it would require more than two thousand of these 'phage particles placed together in one plane in a circle to bring them within the range of visibility. Whether or not this infinitesimal size is in itself sufficient to place this particular virus in the realm of non-living substances, I, for one, will not venture to say. I shall return to this question later.

When we review measurements made on other ultrascopic viruses, particularly those responsible for certain animal diseases, comparatively few results present themselves for consideration. The reason for this probably lies in the fact that the obstacles which have to be overcome here are much greater than those which apply to the bacteriophage. Bacteriophage suspensions present comparatively little in the way of associated colloidal matter to disturb the results. Even these may be removed without great difficulty, and it is largely because of this fact that succeeding investigators have been able to assign progressively lower orders of magnitudes to the bacteriophage.

<sup>1</sup>  $\mu$  = millimicron = 1/1,000 of a micron. If we remember that a micron is equal to 1/25,000 of an inch or 1/1,000 of a centimeter, and that a spherical body 0.5  $\mu$  in size may just be seen nicely with a good microscope, we can picture roughly at least the size of a  $\mu$ .

In the case of plant and animal viruses, by far the larger portion of a virus suspension is made up of disintegrated host tissue substance. This is easily understood if one remembers that in preparing the virus suspensions one grinds the affected tissues containing the agent to a finely divided state, and it is this mixture which one is compelled to work with in a study of these viruses. The foreign material with which the virus is associated tends to clog the pores of even the coarser collodion membranes, reducing their size, which in turn has the effect of giving to the virus a larger particle size than that which it actually possesses. The disturbance caused by this colloidal matter is not due solely to the physical aggregations represented, but in a large degree to the electrical charges which these aggregates bear, causing the viruses also bearing electrical charges to become strongly adsorbed to larger aggregates.

While some progress has been made in effecting a partial separation of this foreign matter from the virus suspension, this has not been sufficient to affect the problem materially. Nevertheless, in spite of these difficulties some suggestive observations have been made on the viruses in question. Enough has been learned to tell us that here also we may be dealing with agents whose physical magnitude may perchance not greatly exceed that of the bacteriophage virus just referred to. Indeed, one investigator has estimated the size of the virus of fowl plague at about  $2.5 \mu$ , a figure which may possibly be a little too low for this virus, taking into consideration more recent investigations. Another investigator found the viruses of cowpox, of rabies and herpes-encephalitis practically as filtrable as the bacteriophage. According to still another set of measurements reported, the virus of foot-and-mouth disease and also that of chicken sarcoma seem to lie well below  $100 \mu$ . Measurements made by us on

the virus of infantile paralysis suggest that this agent lies somewhere below  $100 \mu$ . This work is still in progress, and it is entirely possible that further results with more favorable virus suspensions will point to an even lower order of magnitude.

The particle size of the virus of tobacco mosaic disease has been found by Duggar and Karrer-Armstrong to be comparable to that of a 1 per cent. hemoglobin solution. These results suggest that the magnitude of this virus may not exceed that of the bacteriophage.

It is probably quite unnecessary for me to stress the importance and significance of the observations reported. Certainly, the results reported thus far point quite definitely to a new group of agents clearly separable from the microscopic forms known to us as bacteria. They must either represent exceedingly primitive forms of life or belong to a class of agents analogous to ferments. That they are not just submicroscopic bacteria seems certain.

#### CULTIVATION

Another reason for regarding the viruses as unrelated to bacteria lies in the fact that despite repeated attempts to cultivate them on lifeless artificial culture media they have never been cultivated in this manner. While there have been a few successful attempts reported, not one of them has been sustained by subsequent workers. Certain viruses (cowpox, herpes) have, however, been cultured in tissue cultures, that is, in the presence of living, growing tissue cells. Thanks to the excellent work of a number of investigators, particularly of Carrel and of Lewis and Lewis, the art of cultivating tissues *in vitro* has reached a high degree of perfection. Not only is it possible to cultivate successfully tissues of such low order as mesenchyme (connective tissue cells, etc.), but also more specialized tissues,

such as endodermal and ectodermal cells, liver cells, skeletal and smooth muscle cells, nerve fibers, etc. Not only normal, but cancer cells from certain tumors of rats, mice, dogs, chickens and of man have also been successfully cultivated *in vitro*. While the procedure is a little exacting, it nevertheless promises to yield itself well to a solution of some aspects of the virus problem. Strangely enough only a few investigators have made use of this avenue in their approach to the virus problem. Thus far, only a few viruses have been cultured in this manner. The virus of fowl plague has been cultured in tissue cultures of bone marrow; the virus of Rous chicken sarcoma in cultures of leucocytes and embryonic tissue cells, and vaccinia virus in a variety of tissue cultures.

#### TISSUE AFFINITIES OF VIRUSES

Not only do the ultrascopic viruses seem to require living cells for their propagation, but many appear to be highly selective not only with regard to the species of animal they will attack, but also with regard to the particular tissue they pick out. The virus of poliomyelitis not only limits its attack to man and monkeys, but exercises a decided selective affinity for nervous tissue, affecting primarily the motor nerve cells in the medulla and cord. Other viruses are more or less equally selective in picking out the animal species and particular tissues. Some are more selective than others, but all of them exhibit a well-defined affinity for certain host cells rather than for host tissues in general.

The viruses not only tend to exercise considerable choice in the type of tissue cell they invade, but many of them exhibit a decided preference for young growing cells. The bacteriophage almost without exception produces its effect on young growing bacteria. The phenomenon of lysis and regeneration of the principle is brought out most clearly when the lytic agent is added to a young, actively

proliferating culture. The activity of plant mosaic virus is manifested only in the younger leaves of the plant. The implantation of cowpox virus in anti-smallpox vaccination is, as you know, facilitated by scarification, an operation which is promptly followed by a regeneration of lost tissue and therefore the appearance of young, rapidly dividing cells. Once a virus has become implanted it may by cell destruction or other means bring about the necessary proliferation of quiescent body cells to meet its demand for young impressionable cells. Such growth-stimulating action is noted particularly in a disease known as chicken sarcoma, a cancerous affection in which proliferation of certain tissue cells takes place in a manner analogous to that observed in other malignant tumors. The addition of a bacteriophage to a young bacterial culture serves to speed up greatly the rate of bacterial multiplication. The cell injury which apparently accompanies the growth-stimulating action in the latter instance is accompanied by a marked influx of water into the cell, causing it to burst much in the fashion of a toy balloon. As a newspaper reporter has aptly put it, in describing the theory of a certain investigator, "The bacteriophage plunges bacteria to an early death as the result of the *fast living* and *too much drinking* which it induces." (While this tells us nothing regarding the exact nature of the agent responsible for this pernicious influence on bacteria, it does convey a vivid picture of the more obvious consequences which follow the addition of bacteriophage to impressionable organisms.) Apparently two forces operate in a virus disease—one stimulating the cell, the other destroying it. In certain virus diseases, as in chicken sarcoma, contagious epithelioma, fowl leukemia and in warts, growth-stimulating forces predominate; in others, such as smallpox, foot-and-mouth disease, herpes, poliomyelitis and

the lysis of bacteria under the action of a bacteriophage, the destructive forces overshadow the cell-stimulating forces. This has the effect that in certain virus diseases we may observe tumor-like proliferation of cells, while in others only cellular debris may mark the places where normally placed tissue cells once existed. While different viruses tend to influence cells more in one direction than the other, it is apparent that in either case the effect is on certain selected cells which the virus presumably actually invaded. While bacteria may flourish in large numbers in the interstices of a tissue, they rarely invade the tissue cells themselves.

#### INCLUSION BODIES

Viruses differ from bacteria not only in their preference for an intracellular existence, but also in that many of them give rise to peculiar formations within the affected cells known as inclusion bodies. In some virus diseases, such as rabies and cowpox, these bodies appear only in the cytoplasmic portion of the cell; in other diseases, such as herpes simplex and Borna's disease of horses, only in the nucleus of the cell, while in still other infections, such as smallpox, in both the cytoplasm and nucleus of the affected cell. These may be round, oval, pyriform or irregular in shape, homogeneous or granular and either acidophilic or basophilic in their staining reactions. Their nature is a disputed question. Some of the earlier investigators regarded them as protozoa, picturing them in various stages of an elaborate life cycle; others have come to the conclusion that they represent a class of micro-organisms which become characteristically coated with cellular material and have accordingly named them "Chlamydozoa" or cloak animals; a few investigators consider them as more or less solid aggregations of the virus corpuscles themselves. The general trend of opinion at the present time

seems to be that these inclusion bodies represent primarily reaction products derived from constituents of the cell rather than the agent itself, though as certain recent investigations have conclusively shown, the virus itself may be prominently represented within the inclusion body. Whatever the exact nature of the inclusion body may be, there can be no question that its presence is definite evidence of a virus infection. On this rests one of the most important procedures in the diagnosis of rabies in which the finding of so-called Negri bodies within the nerve cells in the brain constitutes a positive diagnosis of the disease. Not all virus diseases, however, are accompanied by these cellular inclusions. Poliomyelitis is a disease in point. It appears that in poliomyelitis the cytolytic changes move along too rapidly to permit reaction products to form within the cell. Exceptions of this sort are, however, relatively uncommon, and it may be that suitable methods still remain to be worked out for the demonstration of similar reaction products for the diseases in which these exceptions seem to exist.

#### IMMUNITY IN VIRUS DISEASES

A significant difference between ultravirus and bacterial diseases is also noted in the relative duration of the immunity which follows recovery. While recovery from certain bacterial diseases (typhoid) tends to leave one with a more or less durable immunity to the same disease, this is not true for the vast majority of bacterial infections. Repeated reinfections with the same bacterial species during the course of an individual's lifetime is not a rarity. In virus diseases, on the other hand, the contrary is the rule. While exceptions may be cited, virus diseases generally leave those fortunate enough to recover with a solid and lasting immunity, an immunity which is real and not merely relative. Why then is there this strik-

ing difference in the solidity and durability of the immunity in these two general classes of infectious diseases? Is the difference to be explained on the ground that the defense mechanism of the cell is more profoundly stimulated by a virus than by a bacterium, or does the defense mechanism differ radically for the two general groups of agents, the one being relatively more effective in carrying out its particular function than the other? No satisfactory answers to these questions are at hand. Recent observations suggest that the acquired immunity against virus diseases may rest in part at least on a persistence of the virus in the tissues of the host following recovery, in other words, may consist of an infection immunity analogous to that which exists in a person, or an animal, infected with the spirochaete of syphilis. It is well known that a syphilitic individual can not be superinfected so long as he harbors the organism. Not until he has been actually cured of the disease, and only then, does he become susceptible to reinoculation. This possible explanation of virus immunity finds support in recent observations which have been made on the persistence of viruses in the tissues of animals following recovery. The blood of a horse has been found infectious seven years after an attack of pernicious anemia; the virus of foot-and-mouth disease has been recovered from a bull six months after recovery; vaccinia virus has been recovered from the tissues of experimental animals several months after infection and recovery. The virus of poliomyelitis has been found in monkeys five months after recovery from an acute attack of the experimental disease. Bacteria which survive the lytic action of bacteriophage and grow up as secondary resistant cultures not infrequently continue to flourish from generation to generation in intimate association with the bacteriophage. Once freed of the principle they tend to

revert to susceptible forms again. Other examples might be cited. Of significance also are the observations recently reported by Olitsky and Long on vaccinia immune rabbits, who found that the acquired immunity following inoculation disappeared in these animals soon after the disappearance of the virus from the tissues of the host. From the various observations cited it would seem, therefore, that acquired immunity against virus infections may swing, in part at least, on a persistence of the virus in the tissues of the recovered individual. What keeps the virus in check within the host is not exactly clear, except that we know that the serum of such an immune individual contains antibodies capable of inactivating the virus. Strangely enough the antibodies which seem to be responsible for holding the viruses in check apparently accomplish this not by actually effacing the agent, but by rendering it incapable of injurious action. We may assume that it is not necessarily actually destroyed because inactive serum-virus mixtures may within certain limits of time be rendered active again by well-recognized procedures, but this brings me to the next chapter in the development of my theme, namely, the antigenic behavior of the ultraviruses.

#### ANTIGENIC PROPERTIES OF ULTRAVIRUSES

If we stop to analyze antigens<sup>2</sup> as a group, we find that they may be divided into two general classes—those which, like toxins and ferments, stimulate within the host the formation of specific neutralizing antibodies; and those which like bacteria and other complex proteins stimulate the formation of antibodies (cytolysins, agglutinins and precipitins) whose function seems directed solely

<sup>2</sup> An antigen is any substance which injected parenterally into an animal will stimulate the formation of specific antibodies—substances which react in one way or other with the antigen.

towards an elimination or removal of more complex foreign proteins from the body. While no sharp line has been drawn between these two general classes, certain fundamental differences seem to present themselves which warrant an attempt to classify ultraviruses in part on the basis of their antigenic behavior. Such studies have been undertaken in a number of laboratories, including our own, with significant results. The sum total of these studies seems to indicate clearly that the antibodies stimulated by the ultraviruses resemble much more closely antitoxins and antienzymes in their nature than they do the antibacterial and the antibodies which are formed against other complex proteins. Expressed in another way, they seem to belong to the first rather than the second class of antigens. Like the neutralization of the soluble toxins formed by the diphtheria bacillus by its antitoxin, a mechanism which has to be distinguished from that which has to do with the elimination of the diphtheria organism itself, antibodies formed against the ultraviruses function merely as inactivators or neutralizers of the virus. In this inactivation the virus does not appear to be actually destroyed but merely rendered inert because of some sort of union which takes place between the virus and the antibody. As in the union of diphtheria toxin with its antitoxin, the union between a virus and its antibody may, as I have already indicated, be broken up with the result that the active virus, as well as the antibody, may be set free again. The mechanism is in a measure comparable to that of an ordinary chemical reaction in which the resulting compounds may be resolved again into their constituent substances. This dissociative effect, which can for a time at least be realized, is something not realized in the reactions which may take place between bacteria and their antibodies. It will be seen, without going further into a technical subject, that the

ultraviruses are distinguishable from bacteria not only in the several respects already mentioned but also in their antigenic behavior. Not only this, but we have in the close resemblance which they bear antigenically to toxins and ferments further evidence of their lowly organization and distinctive properties. I should add, however, that while the latter is true, this does not justify the conclusion that the viruses are actually of the nature of true toxins or ferments. All one can say is that the ultraviruses, because of distinct and singular differences in the chemical and physical constitution, seem prone to elicit an antibody response differing fundamentally from that which is elicited by more highly organized proteinaceous bodies.

#### NATURE OF THE ULTRAVIRUSES

From the facts brought into review we can see that the ultrascopic viruses represent a class of infectious agents whose properties differ in certain important respects from the simplest forms of microscopic life known to us. We have seen that they differ not only in being of a much lower order of magnitude, but also in their inability to grow on lifeless artificial media, in their dependence on and selective affinities for special tissue cells, in the peculiar cytological changes which they induce, in the type of immunity which they give rise to and also in their antigenic behavior. What, then, is the exact nature of these filtrable agents? Do they represent exceedingly primitive forms of life or are they perchance non-living agents of the nature of ferments, hormones or toxins—substances which set up within a living cell disturbances of such a nature that the affected cell is caused to elaborate more of the selfsame agent? Both views are being entertained, and one may, in the present state of our knowledge, argue quite as effectively for one view as for the other.

Possibly the best argument in favor of the living nature of these ultrascopic agents is their transmissibility in series, a phenomenon which one feels forced to associate with a progressive multiplication of the virus particles. It is, however, not necessary to assume that this multiplication can take place only as a result of some sort of cleavage or reproductive mechanism inherent within the virus corpuscles themselves. It is barely possible that such a regeneration might be accomplished through the medium of the host cell deranged in some way by an appropriate agent setting up a disturbance of such a nature as to cause the cell itself to elaborate more of the selfsame principle capable of affecting new cells. While the provision of such a mechanism of virus regeneration would seem an unusual thing to expect of a cell, whether diseased or normal, this possibility is strongly embraced by some investigators. True, there are no satisfactory parallels known to biologists on the basis of which such an assumption can be reasonably supported. That damaged tissues liberate a growth-stimulating principle has been known for some time. In such a scheme of things the newly formed cells are potentially fully as able to liberate this principle on being injured as were the original cells which they displaced. Yet in this scheme we do not have self-perpetuating cell destruction going on hand in hand with that of growth stimulation. Were one able to reconcile in any way the regeneration of self-destructive agencies with that of the biological struggle for self-preservation, it would be a little less difficult to leap the gap which exists between such contrary tendencies in nature. Something which does harmonize with the idea that viruses may be regenerated by the affected cells has been observed in studies on certain ferments. Bordet and Gangou, while working on coagulation of the blood, observed that a given quantity of thrombin not

only exercises a direct coagulating action on blood, but also stimulates the production of new thrombin at the expense of materials in the coagulable fluid. A plasma which no longer yielded thrombin spontaneously could be made to yield considerable quantities on the addition of a small quantity of thrombin. Another investigator has found that enterokinase produces a similar transmissible effect in the transformation of inactive trypsinogen into active trypsin. Not only does a small amount of enterokinase serve to activate a given quantity of trypsinogen, but the products of the reaction may serve to activate new quantities of trypsin. These observations, while not strictly comparable to the transmissible effects produced by viruses, nevertheless offer something for our consideration.

The possibility that the ultraviruses may be of a non-living nature tends to be supported also by certain observations indicating that they may be caused to arise *de novo* either naturally or under the influence of certain agencies. A number of investigators claim to have recovered bacteriophage from bacterial cultures previously free of this agent either after allowing them to age under more or less natural conditions or after subjecting them to certain unusual physical and chemical influences. Similar observations have been reported bearing on virus diseases of animals. According to Carrel the injection into chickens of embryonic tissues and certain chemicals, such as arsenic, coal-tar, indol and peptone, produces sarcomas which contain the Rous sarcoma virus. Similar results have been obtained with spleen tissue treated with arsenic *in vitro*. Rivers and Tillett chanced to uncover a new virus by injecting intratesticularly suspensions of ground rabbit testicles from rabbit to rabbit in the course of another investigation. Naegeli has noted the appearance of herpetic lesions in a certain percentage of

patients injected with bacterial vaccines. All these results, however, bear the stamp of chance findings and are, as subsequent investigations have shown, not readily reproducible. Until one can make certain that viruses have not been accidentally introduced from outside sources, conclusions on the basis of such observations must naturally be held in abeyance.

Of particular interest in connection with the question of the nature of ultraviruses are the recent observations of Vinson and Petre on the virus of plant mosaic disease. These investigators were able to precipitate this virus from the juices of the diseased plant by means of aqueous solutions of safranin. In the precipitate the virus is apparently held in an inactive condition but can be reactivated again by removing the safranin by means of amyl alcohol. By further chemical procedures they were able to get the agent in a comparatively, though not entirely, pure state. They concluded from their studies that the behavior of this virus is in many ways analogous to that of a chemical substance.

While there are good reasons for doubting the living nature of at least some of the ultrascopic viruses, there are also excellent reasons for regarding them as animate bodies. Were we better informed as to what actually constitutes *life*, there might not be any cause for controversy as to the nature of the agents under consideration. Since we have no satisfactory answer to the fundamental question of "What is life?" it becomes necessary for us to get on as best we can with the criteria at our disposal, remembering that these may only partially measure the essential attributes of life. We must remember also that the criteria we may see fit to measure by are based entirely upon the behavior of complexly organized living bodies, even though based upon the sim-

plest single cell organisms with which we are familiar. We know nothing whatever regarding the fundamental attributes of the ultimate living or life-giving constituents of the cell. We are entirely uninformed as to how the attributes of these ultimate units differ from those of the living cell as a whole. And this is one place where our method of measuring life begins to break down. That bacteria, at least some of them, may become transformed into filtrable ultrascopic stages and back again to visible forms seems to have become more and more clearly established. So far as we know the attributes of the units in the ultrascopic stage may be totally different from those of the microscopic forms under observation. But I must not allow myself to be carried too far afield. Rather, let us make use of the criteria of life so far as they are known to us. Let us see what they are and to what extent they may serve our needs in the present problem. Probably the best consideration of this question is offered by d'Herelle in supporting his hypothesis of the living nature of the bacteriophage. D'Herelle believes that the essential criteria of life are (1) the power of assimilation in a heterologous medium and (2) the power of adaptation or variability. In applying these criteria to the viruses, the answer to the first question swings on a second, namely, that of the autonomy of the agent under consideration. If it can be proved that the agent in question is actually autonomous—is an agent definitely foreign to the host—then the very fact that it is transmissible, in series—that is, multiplies—proves that the agent must possess the power of converting host substance into its own substance, in other words, possesses the power of assimilation. Proofs of autonomy may vary in character. None are easily established. D'Herelle has offered what he regards as at least ten

different proofs of the autonomy of the bacteriophage. I have time to refer only to two which, to me, appear especially significant. If one injects a rabbit repeatedly with a bacteriophage lysed culture, specific antilytic or antiphagic antibodies will make their appearance in the blood of the animal, while the injection of a 'phage-free bacterial culture, whether autolyzed or not, never gives rise to such antilytic antibodies. The bacteriophage, therefore, appears to possess distinctive antigenic properties distinguishable from the antigenic components of the bacterial cell. In other words, antigenically it behaves like an independent autonomous body. Moreover, if we take an antilytic serum so produced and add it to a secondary culture, one in which resistant bacteria and 'phage coexist "symbiotically," and in consequence of which the cultural properties of the bacterium have been profoundly changed, we will not only succeed in inactivating the bacteriophage in the mixture, but we will also succeed in converting the cultural behavior of the bacteria back to their normal state. This "curing action" of antilytic sera on abnormal secondary cultures may well be compared to specific serum therapy in infectious diseases of man and animals. It is an effect which one can easily understand if he regards the bacteriophage as an autonomous agent, foreign to the bacterium attacked. It is, on the other hand, an effect which is difficult to understand when one views the bacteriophage as some abnormal product of the bacterium's own body. What I have said regarding the autonomy of the bacteriophage probably applies equally to other ultraviruses. With the autonomy of a virus fully established, a matter that is more easily affirmed than done, there seems to be no other conclusion left for us to draw than that the viruses possess the powers of true assimilation—one criterion of life. It has

seemed to me for some time that the only logical manner for d'Herelle's opponents to attack his hypothesis of the living nature of the bacteriophage would be for them to undertake to test the validity of the hypothesis on the basis of the criteria of life advanced by d'Herelle. How secure is the evidence that the bacteriophage actually exhibits the two basic criteria of life—assimilation and adaptability? What evidence can be amassed to prove that the bacteriophage is not actually autonomous, that the proofs advanced by d'Herelle are more apparent than real? What evidence can be collected to show that the bacteriophage does not in truth possess the powers of adaptation? Strangely enough most of the work of d'Herelle's opponents does not relate to these questions. I can say this with a spirit of full neutrality, for to me the question still remains an open one. In sympathy with my view that the validity of d'Herelle's hypothesis should be tested in terms of the criteria of life advanced by d'Herelle, Dr. Beard, a colleague of mine, undertook to make a careful study of the capacity of the bacteriophage to adapt itself to new environmental conditions, particularly of its ability to adapt itself to the attack of entirely new bacterial species. The results of these studies, which he carried out in a very critical and painstaking manner, forced Dr. Beard to conclude that the bacteriophage possesses the power to adapt itself to new bacterial species only in so far as it may possess at the outset a certain "latent" capacity to attack the new species on which it is being trained. Only in the event a bacteriophage exhibited slight or virtually hidden capacity to attack the new species could this capacity be enhanced by serial passage on the new species to a point where the 'phage more or less energetically attacked the new species. In the absence of such "latent" capacity, adaptation to new bacterial species

did not occur. While he was unable to duplicate the results of certain other investigators with respect to the facility with which 'phages were claimed to adjust themselves to new bacterial species, previously not attacked, there nevertheless still remained unexplained the capacity of a bacteriophage to increase from a very weak to a very strong 'phage. This increase in activity, or increase of virulence, reminds us not only of similar properties exhibited by other ultraviruses, but of similar capacities exhibited by such definitely known living organisms as the bacteria themselves. It is a form of adaptation with which every pathologist is familiar. The question before us is then this. Is this enhancement of the virulence of a bacteriophage based upon the same fundamental law as the enhancement of virulence of pathogenic microbes, definitely known to be living? All we can say is that outwardly the two phenomena resemble each other very closely. Basically they may, for all we know, differ widely.

Some doubt has been expressed as to whether bodies as small as 5 to 10  $\mu$  could still be complex enough to be living. In the first place, it must be remembered that the measurements at our disposal represent only rough approximations. In the second place, as I have already pointed out, we know nothing whatever as to what actually constitutes the ultimate units of life. As Gaskell has recently suggested, "life" may be an "intraatomic quantity." It may, on the other hand, be something that depends upon more complex physical aggregations. Of this we can be sure, that the single-cell organisms we see under the microscope represent in reality quite highly organized forms of life. In them one observes already considerable specialization both in structure and function. The smallest visible cell, with no nucleus discernible, must still have represented within its body a diversity of

substances which determine the varied physiological processes we can more or less measure. Hofmeister has estimated that a liver cell contains in all more than 200,000 billion molecules, among them 50 billion protein molecules,<sup>3</sup> 150 billion lipoid molecules and 2,000 billion crystalloidal molecules of lower molecular weight. A cube of 0.1  $\mu$  or of 0.001  $\mu^3$  (one thousandth cubic micron) volume, that is, a structure of effective submicroscopic size, could, according to this, still contain 25 million molecules of water, 25,000 colloid (protein) and 250,000 crystalloidal molecules. Though we may acknowledge that a body of just submicroscopic size may still present a fair number of protein and other molecules, when we drop to bodies measuring only 5 to 10  $\mu$ , the number of protein molecules which are still possible becomes negligible. A coccus of 10  $\mu$  has been estimated not to contain more than twelve molecules of the size of serum albumin. But why should we disturb ourselves over the question of how many protein molecules may be necessary to establish the simplest living unit? The ultimate unit of life may not hinge at all on the question of whether protein is present or not. For all we know the ultimate principle of life may antedate the simplest protein molecule. It is conceivable that from such primary living units may evolve first the smallest living micellar aggregates; from these primitive aggregates the simpler microscopic cells may eventually evolve; from these in turn the more complex unicellular organisms, and with still further specialization and division of labor these give rise to the simplest multicellular organisms and so on. In such a scheme of evolution it is entirely conceivable that a place could be found for the ultraviruses quite regardless of their physical magnitude. Who can say?

<sup>3</sup> To the protein molecule he gave an average molecular weight of 16,000.

## HOW DID WE COME BY ART?

By Dr. WALTER HOUGH

U. S. NATIONAL MUSEUM

PHILOSOPHERS, notably Bacon, have viewed man in his beginnings as a helpless creature. This conclusion may possibly be inaccurate, but it does not require an effort of fancy to portray early man as bereft of superficial graces. Man had not the finish bestowed by nature in exquisite beauty on insignificant organisms. He seems to come upon the horizon of life as an afterthought, with beetling brows and rough, dark, hairy skin, a very ugly duckling.

He is provided with strength of body and cunning of brain, serving to put him first in the competition of nature and urging him forward to the culmination when he shall have all things. But in his early stage we do not know that he appreciates his surroundings of natural beauty, nor is it imaginable that he perceives such things as flowers, the brilliant plumage of birds, their songs, or the lovely tints of wet sea shells.

Man is thought to have emerged from a gnarled stem of ungraceful mammals and surely destined from the beginning to make his own way to the decorative perfection which ages of development had given to other animals. His incompleteness is still striking; he intrudes and devastates, yet looks forward to when the scars will be healed, hundreds of millenniums in the future.

In the past we must see him as tentatively feeling his way toward the secrets of his relations to his surroundings in the visible world, the first animal that grasped the keys that would open a never-ending progress. Why and how he did this we would like to know, fascinated, but feeling that these questions can never be answered.

We know, however, that from these fertile beginnings he would build art

exterior to himself, a vast realm that nature never knew, exceeding anything ever touched by life, and foreign, so far as we with our limited understanding may guess, to the processes of that mysterious force animating living beings.

In this he would arrive at, by other incentives, the clothing of his ideas as nature decorates her life forms. It is a fair theory to put forward that man in clothing the invisible world with symbolic dress began the long course of art. Nothing exists to show the faint beginnings, what they were like or in what order they proceeded from the inner consciousness of his being, yet somewhere along the line of what seems almost insensible progress the germs are tangibly revealed.

The expression of feelings by muscular movements in figured facial and bodily changes recognized by onlookers as signs of a mental state is universal in the animal kingdom. The old observation that "actions speak louder than words" may be applied here. Out of the animal stage in which such actions are considered psychological emerge the gestures of man taken to be symbols connoting ideas and in effect fundamentals preceding and developing with language. It is evident that gestures universally accompany the complex of speech as a shadow aid, and that they slowly and with difficulty lose their importance.

There is a long period of gesture symbols during which the way is prepared for other forms. Work with the hands in stone, wood, skin, bone, ivory and other naturally occurring materials prepares the media for the recording of art. Without media the groundwork for art is non-existent. The importance

of media is seen to be great in respect to the scope and character of art.

In the order of forms and surfaces art has progressed to the two great divisions—sculpture and decoration. Nature furnished unconsciously the agents by which animals should function to procure food, shelter and the like. By unconsciously is indicated inheritance, development, piling up of habits from experiences, and the vast, mostly unknown part filled with the results of the interaction of life and environment. At some line of fusion man breaks away from the constraints of the intimate surroundings and begins to use extraneous aids to his physical and mental complex. Here at the beginning of arts which are regarded as man-like bed rock is reached.

Theoretically, this period marks the entrance of form as a human contribution to art. Practically, the first expressions of form preserved in enduring flint in the controversial Eolithic period are humanoid only and not to be judged by any conceived tenets of art or even utility, but no doubt satisfying to the Eolithic being, himself mostly a natural product, as conforming to the shapes riven out by usual causes.

With the pre-Chellean and Chellean comes a half natural, half artificial implement with the idea of mass and the beginning point, a feature so important practically and ideally to future arts. From this apparently rapid development of arts, but really of extreme slowness, comes the procession of increasingly concise forms shadowing forth sculpture. Surviving hard materials demonstrate the use of antler, bone and ivory, requiring other freer methods of work, suggesting a wider range of forms which would emerge into the round.

From this point we have predetermined progress that at intervals rose high and again to a low state, yet in essence a real series of advances. Some-

what anomalous developments took place, as that characterizing the school of artists of cave painting in France and Spain whose virile products have formed the life work in presentation by the Abbé Breuil and other distinguished archeologists.

Unique and presumably fostered by some unknown genius, this school of decoration comes into view out of its time and fades away for millenniums till slow development brings in corresponding phases at the proper point in the general line of art. There is nothing in the climax of cave art to show a progress in which symbols played a developmental or originating rôle, despite the thought that paintings were symbols, or rather had a symbolic meaning. For this reason it may be asserted that elemental symbols as discrete units came only with a considerable progress in culture. At early stages symbols had no audience and thus could not have the currency given at a later period when cooperative men had come on the scene. The philosophical view of the symbolism of all things extraneous coming under the purview of human consciousness is not here considered.

Symbols are the shorthand of ideas, the first terms of an equation which reaches an intangible solution in the mind. They are foci for crystallization of thought, human artifacts whose intent is suggestion, which are meaningless to the uninitiated but become the ratiocinative media of the human race. Symbols inaugurate decorative art. They have first currency apparently at a point in culture where abstract ideas can be appreciated.

The idea of property emerges as one of the early manifestations by man of an interest in outward things. While an early workman might not extend the idea of property to flakes struck off in the manufacture of an implement or to rejectage due to any other arts, yet the

finished product becomes his own. If at this state of comprehension the artifact is to be recognized as personal property by others it is logical to surmise that the owner would put his mark on it. This mark is recognized as an early symbol.

In the Old Stone Age the criterion of "mine" would doubtless be a proto symbol revealed in some color, flaw or even habit of flaking which would identify the object. Necessarily we have nothing distinguishable in the stone artifacts that have fallen ages ago from the hands of their owners and there remain none of the perishable elements of social life surviving upon which to note marks or symbols which may have been used.

The steps in psychological phenomenon made by man in producing such simple marks as the circle, square or four-arm cross are perhaps unknowable, falling no doubt into the limbo of unanswered queries as to the origin of many things wrought by the mental equipment of the coming man being.

Ingenuity is no answer because that trait refers to work with things close to the material world. The most that may be said is that at the proper stage these apparently simple marks seem to flash out without preparation or perceptible preparation in anything that has gone before. This is not regarded as dealing with the obscure question of independent invention which comes to the front in a later state of progress. Thus from the point where symbols began to the point where these devices were consciously and customarily used it appears not possible to take up the subject of independent invention, not only from lack of data, but from the obtrusive question as to whether there were any data.

Considered as the simplest form, the straight line is usually given precedence, but apparently quite wrongly. A simple straight line connotes nothing. It is the zero of decorative art and as in

the zero of mathematics was not discovered as a factor for ages. Thus the straight line in symbols of beginning decorative art is positional in relation to other straight lines, that is, two lines cutting each other, forming a diagonal or axial cross.

The development normally of decorative art seems to be with elements of the simplest form or *unit symbols*; intermediate forms or *combined symbols*; arriving finally at complex forms or *design*. As suggested, this evolution does not everywhere follow the normal course. Apparently from a given impetus the course of decorative design has been modified by racial or individual genius. Thus in one area there is a rapid development from a simple motive, and in another it is long and steady, from several incorporated motives gradually becoming wide-spread.

Viewed historically there is a constant flux between freedom and formality in art. It is customary to consider formality a decay from freedom, as a departure from an ideal. In particular this transfer has happened many times even in the most conservative periods among races, but must be regarded as sporadic, while the general course of the growth of the art instincts from the Stone Ages to the eras of achievement has been prevailingly from simple to complex. Thus the theory that decorative art begins with symbols and sculpture with the forms of surviving stone implements seems valid.

#### USE OF SYMBOLS

Property marks have received much attention as to their bearing on the origin of the alphabet. It is possible that these symbols entered into decorative art. Property marks are first noted as prevalent at the period of land- and sea-borne commerce, which began presumably in the Neolithic as an established social feature.

The theory is that traders adopted marks of their own invention for identifying such packages of goods as they ventured in the rude commerce of the times and that gradually these marks assumed a phonetic value furnishing the elements of an alphabet in a later period.

There is much to commend this theory, as it is logical to present a case for the marking of many kinds of personal property acquired during the increasing complexity of social life characterizing the Neolithic, Bronze and early Iron Ages.

Incidentally, evidence of this out-of-date illiteracy is seen far into modern times in the use by the folk of personal and family symbols and in the conventions of heraldry. A most illuminative example of the merging of symbols of various employment into decorative art is observed in Japan. Other countries present similar phases. An interesting legal survival is seen in the X which is "his mark."

There is abundant evidence of the practical and wide-spread use of symbols. It can not be proved that symbols are of any great antiquity; in fact, the weight of evidence is that they are the product of periods of pronounced culture advance. Nevertheless there is a feeling that these devices may have been used, though sporadically, in quite early times and some elements filtered down and were incorporated into the art of later times. Symbols, however, kept clear of art when used as economic factors—witness the English trade-marks sanctioned by Parliament 600 years ago.

While observations show that symmetry is at the basis of all art, it is rare that a purposeful analysis to determine a law of proportion has been experimentally worked out. The analytic mind of the ancient Greeks attacked this problem and was satisfied to accept the style which must have seemed bizarre

at the time. Contrasted with primitive metrology, of which it was doubtless the resultant through a chain of fortunate circumstances, the Greek symmetry can only be regarded as a product of intellectualism too rarefied for continuance, but marking a climax of achievement.

#### SPREAD OF SYMBOLS

It has been customary to think somewhat loosely of symbols as a sort of currency which can be sequestered, borrowed or dispersed through any of the happenings to races between peace and war. Really, the great bulk of symbols are closely bound to tribal art, being worked into design as part of decorative invention, and at least in the later stages rarely occurring in a free state connoting a single idea.

Some symbols considered primary seem also to have become widely diffused. One of these, the swastika, is wide-spread and has been thought to indicate racial connections over great areas. The swastika is in technique based on the unit axial equal-arm cross symbol and formed by the addition of short lines at right angles, the whole symbol intended to convey the idea of motion.

It would appear that the motion concept is the first composition factor to emerge in decorative art. It is seen in most compositions on the axial cross. A remarkable series is observed in ancient Pueblo pottery in which few four-part designs are balanced or static.

The wide diffusion of the axial cross makes it possible that from this symbol alone the swastika originated in many different art areas and this weakens the idea of diffusion by migration. The axial cross is important as a generalization obviously on position of the individual with respect to the external world which divides itself into quarters in accordance with celestial phenomena, that is, the rising and setting of the sun and the opposite points. In decorative de-

signs "here" is a round or square area from which the four radial lines project.

It is not necessary in some cases to begin with a formal symbol because as in the evolution of a Greek fret, from alligator designs of ancient Colombian art shown in the classic study of Chiriqui design by W. H. Holmes, both the square cross and the swastika are transmuted animal designs. In native design animal motives overwhelmingly predominate while floral motives are almost absent, being in the generality of observed cases the stigma of decay.

The circle is also a protean symbol to which various meanings have been assigned. Theoretically the circle originates from the square, and both may arise as a resultant of arrangement of design. The circle is wide-spread, but this is not evidence of cultural spread. The circle with a dot in the center may give evidence of transmission by borrowing in some cases.

The simple circle in design has often followed the use of the tubular drill, and it is also thought that shaped flints were used in the sense of a compass for evolving the figure.

Among the numerous free symbols that give evidence of the fertility of design among various races there are few known to have been borrowed in an extensive way. Mere contact of tribes appears to show borrowing, as the Navaho adoption of some Pueblo symbols, while the latter are coextensive with the pueblo region.

#### MUTATIONS OF SYMBOLS

It is a common observation that there is a constant play of decorative fancy producing general and often profound changes in the units of design. In the life of a symbol changes occur, often rendering the basic symbol unintelligible, and sometimes after a long series of developments the initial form reappears.

Thus a design may begin with comparative realism, show a merging into geometrical or conventional, become entirely conventional and gradually play back again to the beginning with an entirely new meaning. There may be fragmentary remains of realistic in a conventional design, such as an eye or feathers, not erased by mutations and surviving as a clew to the surmised original motive. In these mutations geometrical breaks into curves, or oppositely, curves become geometrical, both sometimes occurring in the same decoration.

The question of primacy of realistic or geometrical is thus confused, the impression being that if realistic begins the cycle it is of very short duration. It is probable that under the designer's exigency the original breaks up into a number of parts or symbols, as the bird design of the ancient Pueblos or the animal designs of the Northwest Coast Indians so thoroughly studied by Boas.

It is probable that the theoretical realistic designs are generally absent and that the geometries begin as lines with triangular or dentated margins, the lines used in combination parallel, converging or outlining wedge-shape spaces. These refer to free-hand work on surfaces as in pottery or skin painting. In basketry and other textiles the mechanical limitations force other treatment, but the mutations are quite as pronounced and varied.

It appears probable that early designs thought to arise out of religious concepts had a permanence not enjoyed by later designs of tribes in more advanced culture. This is in accord with the suggestion that tribes moving into a new environment tend to form new "schools" of decorative art. There are thus many beginnings, often with different origin impetus in motives as is the case with language. Language, as Herbert Spinden once said, is only an-

other form of art. Thirty-five hundred languages in process in the world would mean that speech is highly mutable and has little regard in formation to the time elements.

Instead of fixity the process of formation would be denoted as the play of language in which perhaps a few brighter individuals might, as arbiters of form, affect the course of style content profoundly.

In art such elements enter. The equivalent of language in the relation and relata of design, that is, the symbols and space components of decorative art, tend incessantly to mutations and even in time or certain circumstances decline and are lost perhaps to revive again. Sometimes the change takes place in the ancestral environment, for example, the loss of the Hopi ancient art pottery decoration and its recent revival noted by Dr. J. Walter Fewkes and the writer. Sometimes the art seems to originate from an introduced conception from a higher civilization, as the Vishnu standing between the horns of the water buffalo in Middle Celebes designs—apparently either not superimposed on native designs or eliminating them entirely. This phenomenon is so common that no other examples are needed.

The important cause of the mutation of symbols is primarily that no one can really copy, and for this reason the reproduction of a design leads to changes in position, line or form under the hand of the artist. Much of this is due to degrees of skill noted in the decorators in observed tribes. It is found that in a tribe of several hundred in which there is a general diffusion among workers of the practice of decorative art on pottery one excels, three produce work of class and fifteen or twenty rank low.

This spread is not observed so fully in basketry when the mechanical element is enforced by the routine of stitches, as stated.

#### CONTENTS OF DECORATIVE ART

Evidently the contents of decorative art for a school or period must vary in amount, becoming richer as habitudes enlarge skill and extend the field. Evidently a school of decorative art may begin with a paucity of motives, in fact it is astonishing what an extensive flowering may proceed from a single motive. Notably, also, is this observed when there is what may be called a useful or economic need, as, for instance, in the decoration of bark cloth, for which there is a steady demand, or pottery, basketry, textiles and articles of skin.

A complexity of motives is not the rule in aboriginal decorative art. Here native designs are retained in purity, additions being evidence of racial dislocations and the inevitable processes mentioned. The contents of modern decorative art are clearly seen as an economic phase in which designs are adopted from any place and time, dissolving and reappearing like the patterns in the kaleidoscope. In this phase patterns have hopelessly lost their meaning, generally retained in aboriginal art.

In purer aboriginal art there is observed the use of a central motive lending itself to curved or formal configuration, together with relata of various sorts which set off or frame the prime symbol, affording the desired completeness or picturation of the design in a given area.

Surrounded by the relata the symbolic motive stands as a pure symbol in curved or geometric arrangement or modified to join with the relata. This is particularly observed in band patterns which seem to arise from the limitations or forcing of the design around the margin of the work, somewhat as the edgings of ancient Greek garments led to lace.

The relata or joining elements may be conjectured as arising in modifications of the lines which carry on the rhythmic

flow of the design through repetitions. There is a tendency of the symbolic motive to break up and mingle with the relata in such a way as to be difficult of identification. These structural lines may be bordered on one side with successive wedge-shape figures applied in a diagonal order or perhaps an earlier method by regular dentation. One side of the line generally is so treated. With these elements a remarkably complex series of designs may be elaborated. In some cases a design may be wholly of the serrated line arranged in geometric or diagonally arranged lines.

In general the pure line is sparingly employed and its use implies a considerable advance in art. Occasionally short lines of various lengths are used to produce a shadowy design or a mosaic effect. This procedure indicates a later and purer decoration.

There is also to be considered the breaking up of symbols into smaller units and the addition to symbols of modifying elements. The former is common in the substituting of a part for the whole or the expression of an idea conveyed by a part, the work being charged with decorative fancy actuated by whatever motives, religious or otherwise, may control. Especially is this seen in the anatomized bird designs on Sikyatki ware worked out by Dr. J. Walter Fewkes and in the Northwest Coast totemic designs by Dr. Boas. It is seen that this effort generally produces less effective designs than the older decoration handling fewer motives, such more or less discrete elements preventing the free flow of design.

Wherever data have been afforded for comparison it is found that in ruder decorative arts there is first the appreciation of a medium or surface, which is the background for design. This is a surface built as in pottery or to be built in process in textiles, or natural or slightly prepared as in bone, ivory or

skin or the human integument. On any of these surfaces is placed a design standing alone with only a silhouetted relation to the background.

This design may be repeated in row order, but there is no interconnection of the designs and, therefore, no problems of the interplay of background spaces and pattern figures. These relations are to be gradually appreciated and worked out in the course of the development of decorative art.

Examination of various grades of efforts shows that the problem of spacial application of designs with relation to the background makes for good or bad design.

Also repetitive motives in apposition almost enforce modification of the surface areas of patterns. Thus by hatching or lining parts of patterns variety is secured, and the designs seem to show the idea of duality as in Pueblo pottery.

Harmony of design and background is an evidence of advanced decoration. Sometimes indeed it is difficult to disengage the pattern and background and determine which is the pattern indicated by the artist. Complex patterns in higher decorative art sometimes show the genius of the decorator in the composition of a design in which beauty, harmony and intricacy are combined. Sometimes also the product may be compared to a problem in higher mathematics.

Some designs are repetitive and have little meaning except their general religious intent, as in band decorations or borders.

Often the designer breaks away from the customary usage and produces a free decoration. This is observed especially in the pottery of the Little Colorado Valley where everything was tried out. This is evidence of a later development when the decorators were freed from the trammels of the older black and white pottery motives.

## CONCLUSIONS

Art, like language, has apparently no prime point of origin or continuity of development. They seem, together with other esthetic and intellectual classes, to be of sporadic growth. The line of development which can be drawn in material culture is not so evident in the intellectual complex. Nevertheless some law, as of population pressure, may fertilize and bring to fruition the elements of this complex, on the whole showing an advance.

Thus we must consider that, as in development, races never arrive at the

same place, there are no comparable stages in art. Environment and other obscure causes do, it is affirmed, enforce similarity in gross.

It would seem that in the human texture, called culture, we must recognize that, as in the physical world, mass action represents behavior rather than the results of the movement of small units which lead us astray as to the general or average trend. In this sense it is necessary to avoid basing the origin and development of art on local phases, and we must instead vision the growth of art on broad and comprehensive lines.

# CIVILIZATION AND THE MIXTURE OF RACES

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WHEREVER the members of divergent racial stocks have come into contact they have associated to produce a group of nondescript hybrid offspring. The social and cultural status of these half-caste individuals is determined by the attitudes of the politically and culturally dominant group. They may be accepted as a lower stratum of the dominant culture group; they may be classed as members of the exploited group, or they may be formed into an intermediate class or caste. For a longer or shorter time the two racial groups may live side by side, in a state of relative separateness, each maintaining a semblance of racial and cultural integrity. But the hybrid population grows by natural increase, by the continued intermixture of the races and by the intermixture of the hybrids with each of the racially separate groups. The uniform results are an increase of the hybrids at the expense of each parent group, the present disappearance of racial lines and the ultimate mongrelization of the entire population.

At the present time the contact of racial stocks is incomparably greater than at any previous time in world history. The rise of modern science laid the basis for a new world order. It made possible a machine industry with an enormously increased production of material goods at the same time that it brought new types of communication and cheap and rapid means of transportation; it made possible and inevitable a world-wide commercial and economic unity that brought into contact and association peoples heretofore widely apart. But it brought also a spread and deepening of popular edu-

cation with a consequent growth in objectivity and a weakening of sentimental and traditional controls; it operated directly and indirectly to the economic and intellectual liberation and mobility of the individual man. The new freedom, in the presence of the marked differential in economic opportunity in different regions, got expression in an unprecedented migration. This led to the contact and intermixture of diverse stocks previously widely separated, to the exclusion and absorption of weaker peoples and to the repopulation of whole continents by the amalgamated stocks.

This wholesale and indiscriminate intermixture of biological strains is, in extent at least, unlike anything before known in the contact of peoples. The conditions of the miscegenation have in many cases violated conventional and formal standards and expressed, or given rise to, serious social disorganization and personal demoralization. The facts as well as the apparent immediate consequences have aroused active and violent emotional condemnation. The general public as well as many social students impute great significance to this amalgamation of the races. The prevailing note in the sociopolitical discussion is one of pessimism: there is fear of racial degeneracy, moral decadence and culture decline; an uneasy and unanalyzed sense of impending racial and cultural disaster. In some cases this emotional attitude has been expressed in formal and legal as well as in popular efforts to check the movement already accomplished or beyond control.

It appears to be fairly well established

as a biological fact that neither inbreeding nor outbreeding has any beneficial or injurious consequences. The experimental manipulation of both plant and animal forms seems to have demonstrated that sound stock may be inbred indefinitely without ill effects and that no ill effects follow from cross-breeding. The characters that appear in the offspring are determined by the presence of genetic factors which come from the immediate parents of the individual form. The hereditary traits that mark the ancestral lines appear in the offspring in definite and predictable ratios quite independent of whether the parents are of the same or of different racial strains. Inbreeding makes more likely the appearance of recessive traits, while cross-breeding decreases the likelihood that such traits will appear. But this is simply a question of the presence or absence of similar heritable traits in the parents and not at all a question of in- or cross-breeding. Neither inbreeding nor cross-breeding can produce any characters not latent in the ancestral strains. On the basis of the biological facts and evidence there is no reason to anticipate anything noteworthy as a result of the crossing of human stocks; it would appear to be a matter of relative indifference. But as a matter of historic fact the crossing of racial strains often has been associated with cultural phenomena of utmost human significance. The amalgamation of divergent racial strains seems always to be accompanied or followed by more or less profound changes in culture and social organization. The facts have attracted much attention and have been variously interpreted, but the actual relation, if any, between the biological fact of race crossing and the efflorescence or decline in culture is not generally understood. Two opposing doctrines have held the field.

One school has occupied the position that racial stocks are somewhat grossly

unequal in degree of native capacity for cultural achievement. They stand to each other in some sort of mental hierarchy. The existence of a superior racial group is a precondition to the appearance or even to the use and maintenance of complex forms of culture and social organization. Creative men can be produced only by a superior race. On the basis of this major premise, the argument runs to the effect that any intermixture of the unequally endowed stocks raises the capacity of one at the same time that it lowers that of the other. The net result of amalgamation is a decadence in racial stock and a corresponding decline in culture status.

The argument in support of this position takes numerous forms—biological, psychological and historical. In detail it is frequently highly complicated and, in the hands of different men and sometimes in the treatment by one man, the most contradictory data are marshaled in its support. The major part of the discussion has concerned itself with an effort to demonstrate the unequal endowment of racial groups. This is central to the whole doctrine. If it be established, the intermediate culture capacity of the mixed group and the mixed-blood elements of composite groups would seem to follow as a matter of course. If, however, this is not established, it is difficult to see that racial crossing is a matter of any racial consequence. So, whether as an initial assumption or as a final conclusion, the effort of the school is to show that culture is a function of a particular racial group. In its Western form, to which attention is here limited, the effort is commonly made to demonstrate that civilization is a possibility for the white race only or even for only some one branch of the larger division.

A demonstration of the superiority of the white racial stock has been often attempted on purely physical grounds.

If the racial groups differ in their degree of resemblance to the simian forms it may be possible to show that they represent stages in evolutionary development. In certain respects the whites are more sharply in contrast to the apes than are other races. This is notably true with respect to brain weight and size, in which respects they overtop the other racial divisions. Assuming a correlation between the size of the organ and the efficiency of its functioning, the inference is immediate: the white racial groups excel the colored races and the north European white stock is superior to the other subgroups of the white division. The supporters of this doctrine commonly extend its scope to include sex and class differences within the given society: women are inferior to men in size of brain and hence in the efficiency of their mental processes; the aristocratic, leisure, educated, professional and other well-fed groups excel the socially inferior groups in the average size and weight of brain. The greater historic achievement of the upper classes and of men over women is taken as a cultural expression of the biological facts.

The Spencerian position just outlined has been supplemented and refined by certain findings of experimental psychology. Recently many different tests have been constructed in the effort to measure the comparative mental ability of individuals. They are designed to separate native equipment from cultural acquisition and measure the former apart from the conditioning effects of the latter. Experimentation along this line has led to the general conclusion that white children are superior in native capacity to children of other racial groups and that the degree of superiority varies from one subracial group to another. There is also said to exist a positive correlation between social status and native capacity. To be sure, there is no complete agreement even among

the testers themselves, and there has been much unanswered criticism of the technique and of the findings based thereon. But the conclusions from the use of this recent device of the laboratory are in substantial agreement with the conclusions of the Spencerian argument.

Various writers, approaching the problem from the point of view of the historical evidence, support the same thesis. They find that the great civilizations of the world have been the achievements of the white race or of the white elements of the populations. "Everything great, noble and fruitful in the works of man on this earth, in science, art and civilization, derives from a single starting point; it belongs to one family alone, the different branches of which have reigned in all the civilized countries of the universe." The Indian civilization was the work of this gifted group, while the Egyptian and Chinese civilizations began with colonies from India. The ancient Greeks and Romans, in the period of their glory, were more Nordic—"Aryan"—than the present-day Greeks and Italians, and it was these superior elements that brought about the efflorescence of classic culture. The modern civilization is said to be the unique product of the Germanic races. "It is a definite species of mankind which constitutes the physical and moral basis of our north-European culture." The modern nations—England, France, Germany, the United States—are expressions of this racial genius; civilization without an Aryan creator is impossible. Often it is claimed that only the Nordic elements of this racial division are of cultural worth: "The less Teutonic a land is the less civilized it is." In those cases where some degree of modern culture has appeared among the non-white races, the phenomenon is understood as a copying of Western methods: it is an imitation rather than a spontaneous native growth

and would go into rapid decline but for the stimulus and model of the really superior groups. But some groups are said to lack even an ability to copy. One modern writer, speaking of the Negroes, insists that they lack the capacity even to comprehend many of the elements of white culture and can of course add nothing to it, though they may be able to use some parts of it.

If racial groups are widely divergent in mental ability and culture capacity it follows immediately on logical grounds that racial injury and culture disaster must follow their miscegenation. A large body of historical data has been marshaled in support of the position. Just as the flowering of the culture peoples was a function of a superior racial stock, so their decadence followed up their miscegenation and the dilution or extinction of the white racial elements. The racial intermixture with the Negroes, brought into the Egyptian population as servile laborers, marked the beginning of the end of a great people. The mongrelized descendants were unable to advance or even to maintain the culture status, and Egypt disappeared from the family of culture nations. Greek immigration, resulting in intermixture with inferior and servile groups and the sterility of superior family lines, increased the relative numbers and dominance of the hybrid and inferior strains and so brought racial and cultural decline. Rome is a repetition of the story of Egypt and of Greece. The original stock was corrupted by racial intermixture, and the mongrelized group lacked the capacity to perpetuate the culture. The brilliant culture of the Renaissance was followed by an era of chaos because the "caste lines protecting the Teutonic aristocracy from blood contamination were broken down."

The cultural distribution of the modern world is made to tell the same story. The mixed nations are the backward ones. The Mexican population is a hy-

bridized Indian group, superior to the native Indian, perhaps, but inferior to the Spanish that mingled its blood with that of the native women. South America, generally, shows the cultural consequences of miscegenation with a physically divergent and culturally lower group. In the United States the native Indians were destroyed and the country repopulated by a north European stock; in South America the races interbred. The present cultural contrast between the North and the South American countries is cited as a consequence of this difference in racial policy and practice. French Canada is another case in point: the numerous cross-bred French-Indian population is superior to the native Indian, but so sadly below the intellectual level of the French that they are unable either to preserve the French culture or to assimilate that of other peoples. Numerous other groups illustrate the result of racial crossing: hybrid groups are everywhere backward and decadent.

To persons untrained in the rigorous logical processes of scientific thought and with only a superficial acquaintance with the body of historic reality the position seems convincing. It is simple, direct and in line with the spontaneous prejudices. Many writers, striving for popularity, have exploited the doctrine to an uninformed and eager audience. There has grown up a considerable body of pseudoscientific literature that stimulates at the same time that it caters to the popular beliefs and prejudices.

A second, opposing, doctrine emphasizes the importance and the cultural desirability of racial amalgamation. It is frequently little more than an attempt to refute the position just outlined; the affirmative doctrine, as often as not, goes by implication rather than by direct exposition. The school recognizes the fact that racial intermixture results wherever divergent groups come into association. But the contamination of a stock by the

incorporation into it of other stocks is looked upon as an occurrence that heightens racial capacity and culture worth. The hybrid offspring are likely to be, or demonstrably are, superior to one or both parent stocks. Racial crossing foretells an improved race and culture. The earlier mixture of stocks explains the present cultural status, and the present-day mixture is a basis for anticipating still greater culture achievements.

Every great people, it is claimed, rests upon a mixed racial base. Whenever in the history of culture there has been great achievement it has been the expression of a hybrid rather than of a pure racial stock. "All historical nations have been of mixed blood." "The rise of culture in Greece and Rome, as indeed in western Europe, was in every case preceded some centuries by the conquest of one racial type by another and their subsequent amalgamation." The achievements of the modern world are the cultural expression of hybrid stocks. England, Germany, the United States—the population of every modern nation—is a composite of imperfectly blended stocks. The culture achievement is a function of this fact. Effort is also made to show that many or all great men are of mixed racial stocks.

The general position is supported by evidence drawn directly from the character and status of mixed-blood individuals and groups in the present-day world. Where two divergent groups associate to the production of an intermediate type, the achievement of the hybrid individuals as well as the cultural status of the mixed-blood group as a whole is superior to one, at least, of the ancestral types. Cases are cited from every area of miscegenation. The mulattoes resulting from the association of Negroes and whites in the United States are superior in status and accomplishment to the unmixed Negro group,

and individuals are often well above the average of both the ancestral groups. With few exceptions every American Negro who has risen above mediocrity has been of mixed racial parentage. A somewhat similar condition appears to exist in other similar areas. The Negro hybrids of South Africa, the West Indies, Brazil and elsewhere are, on the average, culturally above the native elements of their ancestry. In some cases these hybrid groups have produced men of real caliber. In nearly every case, the American Indians who have participated in the European culture have been men of mixed ancestry; apparently no American Indian of full blood has risen above mediocrity in, and measured by, the culture standards of the dominant group. The French-Indian hybrids of Canada are individually and as a group above the culture level of the Indian ancestry. The *mestizos* of Mexico and Latin America, the hybrid Eskimos, the Hawaiian and Philippine mixtures and numerous other minor groups of hybridized stock are in culture and social status above the level of one at least of the racial ancestors.

The same general position is supported by a body of negative evidence. The population groups in the modern world with the highest approximation to racial purity are just those groups of most meager cultural accomplishment. The fragments of primitive groups still living are the purest in blood and the lowest in culture of existing populations. In America the white stock with the lowest index of recent racial intermixture are the southern mountaineers. They are at the same time the most culturally retarded white group in the American population.

On the basis of such selected cases it is possible to maintain with some show of evidence the position that culture is dependent upon the intermixture rather than upon the purity of racial stocks.

The two positions stand in more or less direct opposition: one doctrine holds that civilization is an expression of racial purity, or at least can arise only in the presence of a great race of purity, and that culture declines with the decadence of racial stock that results from the intermixture of races; the other asserts that civilization is a result of the mixture of races, that the amalgamation leads to racial virility and cultural efflorescence and that purity and inbreeding of stock lead to racial degeneration and to the decadence and sterility of culture.

Such conflict of doctrine must have a basis in something more fundamental than a simple difference in the reading of the facts; confusion of thought is not persistent in the presence of examined assumptions and rigid definitions. It is desirable, therefore, to turn from the positions occupied to the major logical presupposition upon which they are both erected.

This presupposition, sometimes definitely recognized and stated, sometimes unrecognized and naively assumed, is that culture is a function of race and grows and declines with changes in the composition of the racial stock.

The position of the racial purists, as that of their opponents, rests upon the assumption that a causal relation obtains between race and civilization. Both give a racial interpretation to institutional growth. Civilization is assumed to be a function of race, an expression of racial qualities. Whether the position occupied be that culture efflorescence is a result of racial purity or that of hybridization, or that culture decline is a result of miscegenation or of inbreeding, the basic assumption is that the culture facts are in some direct way determined by the fact of biology. The tenability of either position, therefore, turns upon the soundness of this assumption. It must be shown that cul-

ture is a function of race, else it can not be admitted that the degree of purity or mixture of racial stock is even pertinent to the discussion.

The origin and persistence of this assumption is interesting and enlightening. It arose and prevailed because it offered an explanation within the comprehension of the simple mind. The spontaneous tendency of the popular mind is to assume a direct cause and effect relation between coincident phenomena and between phenomena that stand to each other in temporal sequence. It is a matter of proverbial wisdom that the naive person transfers his emotional reaction from a bit of unwelcome news to the carrier of the information. The whole body of folk superstition is an exemplification of the same type of logical error. Where a direct causal relation actually obtains between coincident phenomena it is not unusual to find misapprehension or even complete reversal of the determining rôle of the coexisting factors: which is taken as cause and which as effect is determined in many cases by the individual or social bias of the observer.

This tendency to assume causality because of coexistence or sequence is particularly pronounced in questions of a biosocial nature. The relation of social and biological facts and processes in the universe of reality is not generally understood. The facts of human culture do not appear or persist apart from the facts of human biology; there are no human beings without culture; there is no culture without human beings. Moreover, the differences between the various racial groups and the various culture complexes are gross and notorious. A similar relation has existed throughout the historic era. It is perhaps inevitable that common sense should draw the inference that the culture complex is a function of the racial variant with which it is associated. The

observation of gross physical differences leads to the position that there are corresponding mental differences which, in turn, are assumed to account for the variations in culture. The prominence of the European Jew in financial circles leads to the position that the group is somehow peculiarly endowed for commercial venturing. The poverty and ignorance of the Negro are commonly accounted for in terms of a native mental incapacity consequent upon the factor of race. The more culturally aggressive peoples of the day are of north European origin; the inference is easy and direct that their cultural status is an expression and a consequence of the racial factor.

This type of common-sense explanation arises spontaneously in the presence of any group of phenomena in spacial proximity or temporal sequence. In biosocial phenomena it tends to persist partly because its flattering implications do not offend the dominant races and classes and are not so subtle as to escape their attention. Moreover, the doctrine puts the ineffective classes and retarded races somewhere outside the strictly human groups to which the ethical imperative applies and so affords a basis and justification for the urge to use and exploit them.

But the assumption persists in part for the reason that the objective relation of the biological and cultural facts has not been adequately emphasized. It is difficult to find anywhere in the literature of biosocial reality a clear analytic statement of the interdependent relationship of organic and social reality. The spontaneous common-sense views, as well as the rationalizations in justification of colonial and other types of exploitative policy, are able to persist because of this absence of a definitive analysis of the relationship of the fundamental processes.

Race, whether the word be used to designate a biological entity or merely a relative biological stability subsequent to intermixture, is a product of inbreeding. The heritable divergences arising in any stock become established as racial marks in the degree to which the individuals bearing the marks are structurally or otherwise isolated. In the breeding of plants and animals to the establishment of a new line, or to the maintenance of a thoroughbred stock, individuals showing the desired characters are separated from others and inbred in order that the traits may be fixed and characterize the strain. Without such separation there is cross-breeding, mixture of characters, the production of hybrid offspring. Exactly the same thing is true of human forms. Purity of race is a result of variation followed by long periods of isolation and close inbreeding of the variant forms. The heritable racial marks are fixed by inbreeding; they are lost in cross-breeding. Without a long period of isolation, whether it be maintained by spacial separation or by conventional barriers, distinctive racial marks are lost and purity of race is non-existent. The only human groups of even relative racial purity are those that have been separated from foreign contacts and inbred through long periods of time. Some of the Eskimos, the American Indians of the Southwest deserts, the interior tribes of Australia, the Andaman Islanders, the hill folk of India, are among the human groups that most nearly approximate purity of racial stock. Their purity is the result of their isolation.

Cultural development, on the other hand, is a consequence of social contacts. Isolation results in cultural uniformity; in stability and fixity of standards; in a régime of suppression, of law and order; in traditional behavior and cultural stagnation. Social

contact means the introduction of new values and methods and divergent ideas. The new may come of course through independent invention and discovery which disturb traditional practices and beliefs and initiate social change. But in general the contact of peoples is a precondition to culture growth. Historically, every civilization has followed upon a period of migration. Every European culture followed upon the contact of different tribal stocks and cultures. The contacts resulted in the introduction of divergent standards and practices and in the breakdown of cultural equilibrium. All progress is made in periods of disorder when individuals are freed because the formal controls are ineffective and the primary controls are conflicting. And change, by contributing to the disorder, is itself a factor in further change.

The two processes thus stand in fairly definite and sharp contrast. Isolation is an essential precondition to racial purity. But the uniform and inevitable effect of isolation is cultural stagnation and retardation. The people most pure in race are most retarded in culture. Contact, on the other hand, is a condition essential to culture growth. It brings the divergent standards, the strange beliefs, the new practices and the fruitful ideas and methods which disorganize the established order and free the individual. On the biological side, the uniform effect of the contact of races is a mixture of blood and the ultimate production of a modified racial type. But the biological intermixture is aside from the cultural development: both result from the contact of peoples

but neither is a direct cause of the other.

The assumption common to both parties to the controversy over the effect of racial amalgamation on civilization is that culture is somehow a function of race. When it is recognized that this position is untenable, that races and culture are independent facts and processes, the whole controversy is without point. Either purity of race or mixture of race may go with either a superior or a retarded culture. Neither racial amalgamation nor racial purity is a causal factor in civilization; neither offers any explanation of cultural decadence.

It is, however, an extreme position to assert that racial amalgamation has no cultural significance. In an indirect way the crossing of races is conducive to social change. In its earlier stages, at least, the intermixture of races takes place for the most part on the outskirts of the civilization. It is in general contrary to the tradition and in violation of the mores; it is usually extra-matrimonial and shocking to the conventional moral standards; it is condemned, opposed, forbidden. Racial miscegenation in these early stages is an evidence and form of social disorganization. It contributes to social disorder, disintegration and confusion of standards at the same time that it makes them evident. It is of course from social disorganization that progress must proceed: change is not possible without it. On the other hand, the social disorder incident to the contact of variant standards and practices is conducive to the violation of the traditional sex tabus, hence is favorable to racial miscegenation.

# FIRE, A PROBLEM IN AMERICAN FORESTRY

By E. I. KOTOK

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AN annual crop of 158,000 forest fires in the United States explains why American foresters devote much of their time and effort to the problem of fire control.

Forest fires, although they have increased in number and severity within the past half-century, are not a recent phenomenon in American forests. They were prevalent through the centuries now past, as a result of both natural and artificial causes. Within historic times we find the earliest European travelers in America speaking of sweeping holocausts that scourged the forests, blackened the skies and drove game animals before them. These reports are not restricted to one locality; they are mentioned in accounts from Maine to Florida, and from the Atlantic to the Pacific. The observations of European botanists who visited this continent in the early days of its history contain some very keen comments on the effects of fires on the flora and forests. Even Dana, in his "Two Years Before the Mast," vividly describes a raging forest fire on the mountain sides near the present site of Santa Barbara (1840). Mark Twain in his own impish way recounts his setting fires in the Lake Tahoe region just to enjoy the spectacle.

But we need not rely on such historic and literary evidence to know that forest fires were common in American forests. The forests themselves offer the best evidence of what has taken place. For example, the fire history in the California pine region has been partially reconstructed from the evidence of fire scars left during past centuries on thousands of trees. Fortunately, in the process of healing, each fire wound has been carefully covered by a layer of woody growth, thus preserving the scar

as a permanent and indelible record of a fire that once covered a given area. Even after the lapse of centuries it is possible to count back the number of annual rings overlying the scar, and to determine the exact year of injury. Boyce, in his study of "Dry Rot in Incense Cedar," which required cutting down in sections thousands of trees, furnished the most complete record bearing on fire scars in the California pine region. This study extended from the Oregon line, through the Sierras, to the southernmost part of the California pine region, and this territorial cross-section gives an adequate basis for drawing a picture of the fire history for a large region. We find in this material that the earliest fire recorded was in 1530. From about 1700 the frequency of scars increases, and the fire history can be more precisely followed.

So it is found that there have been bad fire years in which fires covered practically the entire area of the pine region. At an average interval of eight years and dating back at least to the end of the seventeenth century, fires ran through the timber, leaving their mark, just as fires do to-day. The years 1685, 1690, 1699, 1702, 1708, 1719, 1726, 1735, 1743, 1747, 1757, 1759, 1766, 1786, 1796, 1804, 1809, 1815, 1822, 1829, 1837, 1843, 1851, 1856, 1865, 1870, 1879 and 1889 were bad fire years, as indicated by thousands of scarred trees throughout the mountains. The shortest period between fires is three years, and the longest eleven for the pine region as a whole.

Huntington's investigations of the *Sequoia washingtoniana* enables us to carry the fire history as far back as A. D. 245.

The query arises, if fires have always

prevailed in the forest and nature has been able to adjust itself in their reestablishment, why do foresters place such emphasis on the importance of complete fire exclusion in American forests? The answer, of course, lies in the fact that the forester is concerned in producing and maintaining maximum values in the forest. Fire is not compatible with this objective. Evidence everywhere indicates that even in the finest virgin forests we do not find anywhere near the volume of cellulose which forest lands under complete fire exclusion can produce. For example, on the best sites in the California pine region, areas carrying an average of 35,000 board feet of timber per acre are considered exceedingly dense and productive forests. These virgin forests represent an average age in standing timber of 200 years. In contrast to this value, we find on 60-year old second growth stands in the same region, where fire has been completely excluded, a production of 80,000 board feet of timber per acre.

The virgin forest as the white man found it was the product of soil and climate in which fire was an important modifying agent. In some regions fire was the major dynamic force which moulded and shaped the character of the forest. Whereas endemic infestations by insects or parasites always affect most seriously the older tree age classes and forward the processes of decomposition of single species, fire takes its toll from all age groups and all species simultaneously, even including the destruction of soil.

Fire in the virgin forests acts destructively in many ways: 1. By burning down previously fire-scarred trees by the process of undercutting the base of the tree so that it can not withstand the mechanical strain placed upon it.

2. By heat-killing, causing either complete destruction of foliage or, more often, the killing of the cambium layer.

3. By burning part of the crown and

reducing the vigor and rate of growth of individual trees.

4. By reducing of vigor, increasing susceptibility to insect attack, and through scars offering opportunities for infections by fungi.

5. By completely wiping out younger stands of trees in the seedling and sapling stage which should form the subsequent crop after the cutting of the mature stands.

6. By reduction of the site quality through the removal of organic material and accelerating the process of erosion.

Even the lightest fire may produce some of these deleterious results. In intense fires, under adverse climatic conditions of high wind and low humidity, or where fuel content has been increased through slashing of the forest, complete destruction over large areas may be expected.

In the wake of each fire nature at once begins its processes of restoration through a series of ecological successions. In these successions the more fire-resistant arborescent species and frequently the least desirable replace the more valuable commercial species. There are, however, many exceptions to this rule, where the climax types have been wiped out and the temporary types include very valuable commercial species. In all these processes nature takes abundant time. The forester, however, is confronted with the alternate problem of maintaining continuous forest values within reasonable time limits. Fire introduces a factor of delay and uncertainty.

So far, we have discussed the problem from the standpoint of cellulose production and the effect of fires on this production. Forests have, however, other important values which are seriously impaired by fire. In the western United States, where forests have peculiarly significant bearing on water resources, a single fire, by the removal of the vegetative cover, may produce disastrous re-

sults to dependent agricultural lands. Our experimental data show that by removing through fire the litter, humus and organic material in the forest there will be an increase of immediate run-off one hundred fold, and of eroded material one thousand fold. Where a forest becomes an important watershed it is obvious that every fire is a threat to dependent agricultural land areas. Time here is again an element of importance. Fires in the past affected the water cycle just as they still do, but nature had unlimited time in which to readjust a balance.

It is safe to say that there are no forest regions in the United States, particularly in the commercial tree belts, that are free from evidence of past fires. From this it does not follow that fires have affected the forest adversely to the same degree in every region. In a recent preliminary study in which an effort was made to determine the relative protective needs against fire for the various regional national forests, an interesting relative scale was evolved indicative of damage to forest values for the important forest types of the United States. While this scale is based on the most accurate information of damage studies available, it can be accepted only as a tentative guide pending the accumulation of more accurate data from far more intensive studies. In determining the degree of damage that follows fire, these factors were considered: the extent to which tangible forest values were destroyed; the degree in which the productive capacity of the forest was reduced; the probability of reestablishment of the forest within a reasonable period.

Part of this tentative scale is included here merely to illustrate how variable the effects of fire may be for different forest types, and also how important the forest type itself may be in considering the fire problem for a given region. The spread in damage between forest types is

considerable, being heaviest in the types with the deepest duff layers and in the more uniform age groups.

The relative damage may be expressed as follows (unity represents highest damage): Spruce, 1.0; White pine, 1.0; Douglas fir, 1.5; Western yellow pine, 2.0 to 4.0; Northern hardwoods, 1.5; Appalachian hardwoods, 5.0; Longleaf pine, 15.0; Sand pine, 10.0; Loblolly pine, 7.0; Shortleaf pine, 7.0; Lodgepole pine, 5.0 to 8.0.

The degree of damage that may follow a fire in a given type is highest in some of our valuable forests, as, for example, the spruce, white pine and Douglas fir.

The forester's interest in fire lies first in the fact that his crops, forage or timber, ready for harvest, are threatened; that his lands may lose in productivity, and these losses impair the value of his property immediately. The forester must have an accurate knowledge of fire to understand fully how it has moulded and shaped the forest as he finds it. On this knowledge can be founded a sounder silviculture, and methods for combating successfully and systematically future threats from fire.

With increased use of the forested areas of this country, and with the development that has followed, forest fires have been generally accepted as a necessary evil. Whole-hearted public support in the prevention of fires, in spite of all the evidence of their destructiveness to our forest resources, has not yet been attained. Strange as it may seem, we still hear men of science offer arguments in our western United States in support of "Indian forestry" and the light burning of our forests—a theory and a practice evolved by selfish interests, misguided into the belief that forest fires bring forth luscious forage, bountiful game and open, park-like forests. Until the forester is able to secure reasonable fire exclusion, the development and use of our western forests will be delayed for untold centuries.

# A THIRD ALTERNATIVE: EMERGENT EVOLUTION

By Professor ROBERT K. NABOURS

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THE doctrine of *vitalism*, which holds that life has its origin and support in some principle that is neither material nor organic, has long been subrosa, timorously or frankly discounted by many people. Certainly, the preponderant opinion among scientists is one of agnosticism, skepticism or outright doubt of the existence of a vital principle, entelechy, psychoid, or any kind of immaterial, initiating force or vitalistic entity as ruling, in any sense, the origin, reproduction, growth and activities of plants, animals and man. This tide of disbelief in the supernatural appears to be rising rapidly, and it is hardly more buoyant without than within the churches; for the more liberal ministers and members seem to be coming to regard the supposition of vitalism, especially in its more flagrantly augural and magical forms, as inconsistent, incoherent and bankrupt.

As dissatisfaction with the vitalistic conception, in its various forms, has grown, many experimenters and leaders of thought have resorted to the exploration and exploitation of the doctrine of *mechanism*, as apprehended by most physicists and chemists and many biologists. According to the mechanists, every event should be computable and predicable; all the parts, arrangements and motions which compose and contribute to the operations of the universe follow the same laws at all times; what appears to be new or mysterious is so only to the extent, and because of its not having been previously, or yet discovered and reported; no new principles or methods of action are ever involved, and the mind or mental state does not have

command over contingencies to any extent whatsoever.

The present exalted position of mechanism has been merited through a procession of discoveries and facts. To whatever extent phenomena have been elucidated, all have been found finally to rest exclusively on materialistic bases. We are still awaiting the results of a single, controlled experiment, or observation, which exhibits facts to the contrary. To cite the vast part of the cosmology which has not yet been explained physicochemically renders these mechanistic prognostics none the less exclusive and indicative. Some of the accomplishments, in broad outline, have been the magnificent results in mathematics and astronomy by means of which, among others, celestial phenomena may be predicted with practically perfect accuracy, courses charted and schedules of tides made out long in advance, and intricate as well as monumental structures planned and erected. Developments in biochemistry and physics, physiology, pathology, endocrinology and genetics, as well as other lines, which are rendering incalculable service to mankind, have undoubtedly been promoted by the discard, at least in practice, of the idea of the potency of the vitalistic supernatural.

The discovery and application of the Mendelian laws of heredity, the coincidence of these laws with the behavior of the discrete bodies, chromosomes, in the nuclei of the cells, and the development of T. H. Morgan's workable linear hypothesis which allocates within the chromosomes the factors responsible for the characteristics of plants, animals and

man have all been apparently in line with the tenets of mechanism. Bateson has also quickened the imagination of the mechanistically inclined by the suggestion of the analogy of the breeding cages and pens of the geneticist as instruments comparable with the test-tubes and mortars of the chemist, and the behavior of the genes (hereditary factors) as resembling the chemical elements in analyses and syntheses. Studies in human heredity, as initiated by Galton and carried forward by Davenport, Laughlin and others, have revealed that such discrete characteristics of man as are available for study, and there is now a long and imposing list, follow definite and predicable rules with as much regularity and accuracy as are inherent in other features of science.

Thus it has come to appear that biological phenomena, until recently regarded as in the hands of a fickle god of chance, or directed by categories of capricious entelechies, might be subject to a considerable degree of quantitative predication. The prodigious array of impressive achievements, hardly begun to be described here, has gone far to increase the conviction that the mechanistic conception, hitherto mainly reserved and restricted to the physical sciences, would also serve to explain vital phenomena.

That extraordinarily beneficent and manifold improvements in the material welfare of mankind have accrued from this swinging of the pendulum from the régime of whimsical, inconsistent and depleted vitalism can not be gainsaid, and on this account the movement is not to be deplored. However, from the standpoint of free-will, or optimistic self-determinism, the materialistic assumption appears to many as contributory to pessimism, and highly subversive of the hopeful and purposeful initiative which to this time has been regarded as one of man's most salutary, differentiating and conspicuous attributes.

The third alternative is the doctrine of emergent evolution, so-named, but not first suggested, by C. L. Morgan, and supported by Alexander, Spaulding, Sellers, Smuts and others, some of whom have given the hypothesis the descriptive terms "creative synthesis," "organicism," "holism," etc. This doctrine assumes that examples of emergence, the incoming of the new, beyond the computable, additive or resultant expectations, though still subject to the peculiarities of the components, are to be observed in practically every feature of the universe, as water from the synthesis of hydrogen and oxygen, common salt from chlorine and sodium, gunpowder from the mixture of sulphur, charcoal and saltpeter, words from the letters of the alphabet, green from uniting yellow and blue and the chord from the combination of musical tones. In the whole web of substances and episodes from atom to solar system, from monera to man, and from antdom to kingdom, in all the implications of the latter word, there is ever and inevitably this incoming of the new which, though depending on the characteristics of the constituents, is incomputable, more or less, than, beyond or below, and never the same in all respects as the accretive resultance of the several component elements.

As in the examples suggested, every discrete feature of living as well as non-living substance may be regarded as superadvenient over, or extraneous to, the mere sums, mosaics or additive resultants of their respective, constitutive parts. The laws relating to and governing the whole are also comparably as restricted and peculiar to it as the laws attributive to and controlling the several constituents are exclusive and limited to them, and those of the one may not be even adumbrative of the others. What could be more surprising than that such a novel substance as water should be the outcome, not the mere sum, of the uniting in a certain way of two such

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gases as hydrogen and oxygen? What an emergence of new properties! If just one more element, carbon, be utilized what an array of emergent substances and forces may the chemist produce for us, including ether, if these elements be combined in the one way, or ethyl alcohol when there is another arrangement of them. If we were not so commonly familiar with the preparation and use of such compounds, so dissimilar are they from, and so completely do their various properties dissimulate those of, their respective component elements, it would be difficult to believe our senses at a first experience with their production.

In order usefully to conceive of emergence, stress must be placed on the incoming of the new, the *supervenient properties* of the various products of combinations and syntheses of atoms, molecules, crystals, protoplasmic cells, organs, hereditary characteristics, color patterns, individual organisms into societies, and functions of cells, organs, individuals and subgroups. Thus they are more than the sums of the attributes of atoms, though still consequent upon their peculiar functions, that characterize the various molecules they make; a protoplasmic cell (example, ameba) exhibits properties that utterly adumbrate those of its constitutive molecules (does water any less?); the mule is more vigorous, and, in most respects, superveniently different from the mere accretion, or mosaic, of the several qualities of the ass and horse parents, and the catalo is far from displaying nothing but an addition or mosaic of the respective characteristics of cattle and buffalo. It might be challenged if there is any object or substance, beyond the protons and electrons, non-living or living, from atom to galaxy, which is not actually an advent of the emergent new. All the pages of this journal for a period of years might be devoted to a still incomplete list; "it is beyond the wit of man to number the instances of emergence."

I have, elsewhere (*Science*, April 11,

1930), lately undertaken to indicate the apparently pertinent coincidence between the supposition of emergent evolution and the more recent apprehension of hybridism. Plant and animal husbandmen have, from time immemorial, noted that the hybrid product from the crossbreeding of individuals of different, somewhat distantly related strains, varieties or even interfertile species was likely to be widely different from, though still depending upon, what might be conceived of as a mere combination or addition of the attributes of the various individual parents used; there were usually excesses in size, vigor and other properties over those obtaining in either parent, and not infrequently the appurtenances of the hybrids were so superveniently extraneous as nearly or completely to dissemble those of the original strains. The inability to predict results from a knowledge of the properties of the parents used has always been a fascinating feature of hybridism. If we were not so familiar with the production of most of our common hybrid plants and animals (and which are not hybrid?) or if we should approximate precision in estimating their several qualities and accessories, as compared with those of the races that enter into their production, the novelty of their emergent properties would be so marked as to amaze us.

There are thousands of named varieties of the common dahlias, to suggest only one more case, and yet it is known that they have all been derived from comparatively few elementary strains. Such examples might be augmented from both biological kingdoms *ad infinitum*. In fact, as biologists generally well know, there is not any higher plant or animal, domesticated or wild, which is not, in some of its most essential aspects, the extraneous emergence of hybridism. It may not be too much to state that hybridity has been the efficient, ultimate agency by which the fortuitous, mutant properties of all

varieties of sexually reproducing organisms, including man, have reached, or may attain, whatever grade of supervenient emergency that they now or may in the future occupy, whether it be among the vast majority that perish, those that are suffered and linger awhile, or the presumably relatively few that through the operations of natural selection survive to carry on.

From the view-point here stressed even mental progression in animals and man may be regarded as so many levels of emergence, and with no more resort to the exigency of vitalism than is required in the consideration of the uniting of substances and the resulting supervenient products to which attention has already been directed. Societal forces, whether among insects or men, are also now recognized by ecologists and sociologists as being incomputable, impredictable, and following laws of their own which are usually different from, though depending on, those intrinsic in the lives and laws of their respective, individual constituents. Since "there is not on the planet a single animal or plant that does not live as a member of some biocenose" (societal emergence), and since each higher organism is, in itself, a supervenient emergent of the hybrid combination of hereditary characteristics and the genetic factors involved are incalculable in numbers, and new characteristics are continually coming into existence through mutations and old ones disappearing, it is not difficult to comprehend at least one thing about the future of organisms on the earth, and that is the futility of prediction or prophecy with respect to the possibilities of any individual or society, whether of plants, animals or men.

Furthermore, from the view-point of emergent evolution "each higher quality (emergence) plays the part of deity to that which lies below it." This rather advanced view is more adequately developed by Alexander in "Space, Time

and Deity" and by C. L. Morgan, to whose reasoned discussions the interested reader should by all means turn. It may not be without the bounds of reason to suggest that this supposition is of the utmost importance, for it may actually render deityship, perhaps of a kind not entirely unacceptable in religion, reasonable and tenable to the most rational and practical minded of men. This may possibly be at least part of the way out of the existing *impasse*, which is admitted on all sides, and without serious violence either to science or religion; a means of approximating "the world as man would like it imaginatively [*even actually*] superimposed on the world as it really is."

The idea of emergent evolution has been received with auspicious cordiality in America, as well as elsewhere. Wm. Wheeler, G. H. Parker, H. S. Jennings, H. H. Laughlin and other leaders in science and thought, besides those already mentioned, have espoused and supported the hypothesis to some extent and in one way or another. In recent publications, Dr. Jennings has hailed "the doctrine of emergent evolution as the declaration of independence for biology." Undeviating allegiance may now be devoted to the experimental method with the living as it is with the non-living. The biologist no longer needs to apologize for not accepting "the prevalent dogma that the only method of learning about the organic is to study the inorganic." What is here implied concerning the liberation of the biologist may as well apply to the situation of any reasonable, educated person. Thus the apprehension of emergence, or emergent evolution, is apparently finding a welcome, in one way or another, among a number of philosophers as the more competent course, avoiding facile, pretentious and resourceless mechanism, on the one side, and crude, irrational and insolvent vitalism on the other.

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# THE REIGN OF PROBABILITY

By Professor WARREN WEAVER

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## I

THIS paper discusses one of the classical fields of mathematics—the theory of probability—tracing very briefly its content and significance. It is hoped that the reader will be convinced that there is no other branch of mathematics which touches science and every-day life at so many and such important points. In fact, it will be argued that the theory of probability plays such a fundamental and such an all-inclusive rôle that one is justified in the paraphrasing of the title of Lord Haldane's recent book, and in the speaking of the "reign of probability."

## II

The theory of probability had an unsavory origin. Nearly four centuries ago a professional gambler, Chevalier de Méré by name, discovered (presumably from his cheek stubs) that the odds were better than even that a 6 appear at least once in four successive throws of a die. His clientele, however, seem to have tired of this particular game, and de Méré tried to invent a new one to stimulate the market. He reasoned that two dice can show six times as many combinations as can one die; and that therefore in  $6 \times 4$  or 24 throws of two dice, the odds should be better than even that a pair of sixes would appear at least once. Unfortunately for his bank balance his reasoning was quite unsound, the number of throws he could have safely bet on being actually twenty-five rather than twenty-four. De Méré consulted the philosopher-mathematician Pascal, who corresponded on the problem with his friend Fermat; and the junior elective, *Mathematics 118* (Tuesday and Thursday at eleven with a third hour to be arranged), was founded.

The early development of the theory was closely connected with such problems, and even to-day the student of the subject starts out by learning how to play poker and how to shoot craps. The probability of an event is defined as the number of cases favorable to the event divided by the total number of equally likely cases. Thus the probability of throwing a head with a coin is one half, since there are two equally likely cases, only one of which is favorable. The probability of drawing a club from a deck is one fourth, since there are fifty-two cases, thirteen of which are favorable. To consider a somewhat more complicated example, choose a letter at random out of a box of mixed type. What is the probability that one draw the letter "h"? It is  $1/26$ , since there are 26 equally likely letters, only one of which is "favorable." What is the probability that the next letter chosen be "e"? It is again  $1/26$ . What is the probability of thus spelling by chance the word "he"? According to a fundamental law, the probability of the occurrence of two independent events is the product of their respective probabilities. Thus the probability of drawing first an "h" and then an "e" is the product of  $1/26$  by  $1/26$  or  $1/676$ . This problem has obvious and intriguing generalizations. Put a Hottentot in front of a linotype machine. What is the probability that he will compose, by chance, Keats' "Ode to a Grecian Urn"? Blindfold George Bernard Shaw and put him in front of a typewriter with interchanged type bars. What is the chance that he would reproduce one of Bruce Barton's uplift sermons?

The entire elementary development of the subject is a process of inventing convenient analytical ways of applying

the definition of probability just stated and illustrated. A mere counting of equally likely and of favorable cases is possible for simple problems. But it is easy to construct apparently simple problems in which it is difficult to carry out this enumeration of total and equally likely cases. For example, an even number of balls is drawn out of a sack containing half white and half black; what is the probability that the sample is half black and half white? Again, two integers are chosen at random: what is the probability that they be relatively prime? Again,  $n$  balls numbered from 1 to  $n$  are mixed in a sack and drawn one at a time; what is the probability that not a single ball be drawn in its proper order? That is, what is the probability that never is a ball labeled "r" drawn on the  $r$ -th draw? The numerical answer to this last problem is, for reasonably large  $n$ , the number  $1/e$ , where  $e$  is the familiar Napierian base 2.71828. . . . This result has been recently rephrased in the following post-prohibition form. "If every inhabitant of Chicago got drunk and went home by guesswork, the chance that at least one would get back to his own house is almost two out of three."

After one obtains, from such interesting but comparatively trivial examples, a facility in the elementary technique of the subject, he proceeds to the study of those more refined and powerful methods which enable him to deal with cases involving a very large number of possible events. Here the addition process of enumeration is replaced by integration, and the student meets the so-called probability curve. One studies, at this stage, the theorem of Bayes, which deals with the probability of the causes of events; and he applies the theory of probability to problems of measurement to obtain a theory of errors. One proves the theorem, due to Bernoulli, which is usually referred to as the law of great

numbers. This law admits that no one knows whether a single coin will come up heads or tails, but states that as one repeatedly tosses a coin, the probability gets closer and closer to one (certainty) that the ratio of heads to total number of trials will differ from one half by as little as one pleases. If a million coins be tossed, one can expect that the ratio of heads to total trials will be near 0.5. In fact, what is the chance that this fraction will differ from 0.5 by as much as 3 per cent.? The probability of this occurring is so small that such a deviation would actually occur, on the average, only once, were every person on earth to perform such an experiment ten thousand million times a second for  $10^{18}$  centuries.

At this point in his study, the student begins to meet problems that sound less like Monte Carlo and more like the laboratory, the office and Main Street.

1. A grocer sells, on the average, 50 boxes of "Cream of Wheat" a week. How many ought he to stock every Monday morning in order to reduce to one chance in ten the probability that he will have to refuse a sale?

2. What is the probability, in an elimination tennis tournament involving thirty-two players, that the best and next best player will actually meet in the final contest?

3. The "Big Bertha" shells fell in Paris at the points indicated on a given map. What is the probable direction of the gun?

4. A company manufactures millions of electric light globes, which are shipped in cartons of one hundred globes. What percentage of cartons and what percentage of lights in a carton should be tested to reduce to one in a thousand the chance that there is more than one defective globe in each carton?

5. Two separate field parties come back to the office with sets of surveying measurements taken of the same distances and angles. How should these data be combined to produce the best result? If two thousand dollars is available for raises in salary, these raises to be based solely on field proficiency, how should the sum be apportioned between the two parties?

6. Data are taken in the laboratory and the plotted experimental points seem to lie roughly on a straight line. What is the best straight line?

7. Curves are drawn showing variation of prices over a long range of years. Do or do not the wave-like fluctuations in these curves possess a truly periodic character?

8. Fifty dogs which have been infected with a disease are given a treatment which it is desired to test. Thirty of the dogs get well, and the rest die. A control group of fifty more dogs is infected but not treated; and twenty-three of them die. Does the experiment give any basis for confidence in the treatments?

9. Three thousand university freshmen are given entrance and intelligence tests in various subjects. Their entire university record is later tabulated along with these test grades. What correlation exists between the prediction of the tests and the actual subsequent performance? Do these data indicate a sound basis for advising incoming freshmen?

10. A state government is setting up a Teachers Saving Act. What income must the fund derive from taxation in order that it be statistically sound; that is, in order that the probability be reduced to a safe level that a series of unusual demands bankrupt the fund?

11. In a fertilizer experiment, one fourth of the total land to be used is to be kept unfertilized as a control. How should the total land be geometrically divided up into experimental plots and control plots?

12. What is the probability that every member of a jury will, in the face of confusing and doubtful evidence, vote wrongly?

13. In the city of Edinburgh 49 per cent. of the girls of school age have fair hair and blue eyes, while in Glasgow 51 per cent. have fair hair and blue eyes. Is this discrepancy large enough to be significant?

14. A given quantity is measured ten times under uniform experimental conditions. One of the results differs from the average by three times as much as does any other result. Is one justified in discarding it?

15. Taking account of the growth for the last one hundred years, what is the probable population of the United States in 1950?

16. White female rats and black male rats, both from a mongrel strain, are bred. Each succeeding generation is produced by choosing white females and breeding them with white males of a pure white strain. What is the probable percentage of black rats in the  $r$ -th generation?

17. In a certain gas at a given temperature and pressure, what is the probability that a molecule will have so great a velocity that it is capable of permanently leaving our atmosphere?

18. Three curves are plotted, one showing the sun-spot activity, one the variation in rain-

fall and one of the variation in the price of wheat. Do these curves furnish a basis for concluding that these phenomena are, to any extent, causally related?

19. Considering the extent to which stellar velocities approximate the Maxwell-Boltzmann distribution which characterizes a perfect gas, what is the probable age of the universe?

One could extend such a list of questions indefinitely. Many such, to be sure, can not be answered categorically. In many cases the theory of probability teaches that there is not a single, but many answers; in other cases, one learns that the data are not sufficient, but one nevertheless obtains all the conclusions that the given data warrant. Even in the cases where answers are impossible the theory of probability often greatly helps to clarify one's thinking by pointing out just why the question is vague or unanswerable.

The questions put above indicate that the theory of probability can be applied to many fields of investigation; and this is indeed true. It is easy to see what is the general basis for this extensive applicability. Man's intellectual life is characterized by a sequence of observations of things or relations, and of inferences drawn therefrom. These inferences fall in two classes: inferences which may be classed as logical necessities, and those which may not. The first sort exists chiefly in books on logic (or on mathematics!). One must have exact and sufficiently extensive knowledge to permit conclusions to be drawn by a mere application of the canons of logic. If the information be not exact, or not sufficiently extensive, then the inferences which it warrants are arrived at through the theory of probability. When one states, "A biped has two legs" (definition); "a man has two legs" (exact and sufficiently extensive information), one is then in a position to conclude, irrevocably and unassailably, that man is a biped. But when one says, "I measured the side of this square ten times and

obtained slightly different results whose average is 13.26 cm. What is the area?"; or when one says, "I am thirty-six years old and in sound health. How long will I live?"; or when one says, "The kettle has been put on the stove. Will the water boil or freeze?"—in all these cases one is attempting to argue from an insufficiently accurate and from an insufficiently extensive body of data. One can not legitimately ask for the conclusion. He can merely ask (and then perhaps vainly) for the probability of various conclusions.

Thus it is clear that we are usually interested in situations where our data are not of such character as to lead directly and unequivocally to our conclusions. So the theory of probability plays, whether recognized explicitly or not, an important rôle in a large proportion of our scientific and of our general activity. This will be made more clear if we consider, even though briefly, some applications of probability theory.

### III

As a first and very familiar illustration, we may note the application of probability considerations to the gamble which every man makes with death. We all know that an insurance company could not risk any considerable proportion of its assets on the life of one man. But so regularly and inevitably do the predictions of probability work out when applied to large numbers that the income and outgo of a large insurance company are more stable and predictable than the income and outgo of most commercial concerns. This application of probability to the basic theory of all types of insurance is an old story to us, yet few realize the size or importance of the institution of insurance. The population of our country is at present committed to pay the premiums on over one hundred billion dollars' worth of life insurance alone, this vast sum being

about one third of our total national wealth. The social significance of this large-scale application of probability theory was recently expressed by Ex-President Coolidge, who said, "The life insurance organizations ought to be a source of great pride and satisfaction to the country at large. They are a stupendous force enlisted on the side of public health, sound finance, good government, economic betterment and moral well-being."

### IV

A great branch of probability theory is the body of doctrine known as "statistics," which is being widely used to-day in many quantitative and semi-quantitative fields of experience. Statistical theories fall under three headings: the theory of sampling, correlation theory and the theory of dispersion. The theory of sampling studies the extent and reliability of the conclusions about the original source which one can draw from a sample. Correlation theory studies the nature and degree of inter-relationship between measurements of two or more attributes. Dispersion theory is really a part of the theory of sampling, and treats of the inferences one can draw from sets of samples concerning the homogeneity of the sources from which these samples are drawn.

The theory of sampling, for instance, considers such a question as this: Shipments of fruit from a certain shipper arrive, on the average, in 95 per cent. sound and salable condition. A certain shipment arrives, and a small sample is examined and found to be 20 per cent. unsalable. If examination of the whole shipment is not feasible, what is a fair price? Correlation theory considers such a question as this: University grades and post graduation salaries are plotted for a large number of individuals. To what extent may a student who earns high grades expect a large salary? Dispersion theory considers

such a question as this: Heights are measured of 1,000 Americans, 1,000 Englishmen, 1,000 Frenchmen and 1,000 Norwegians. Does the dispersion existing within these sets of samples indicate that height is attributable more to racial or to individual circumstances?

It is clear that such techniques are valuable to a considerable range of investigators. The educational psychologist, the biometrician, the geneticist, the economic student of prices and price trends, the political economist who is interested in various theories of representation and of voting, the doctor who is studying epidemics or the effectiveness of various preventive measures such as inoculations and vaccinations, the ballistics officer at the front, the efficiency engineer in the great industries—all these must make use, at every turn, of the theories developed in probability or in statistics.

## V

Let us consider in somewhat more detail such quantitative sciences as physics and chemistry. First of all, quantitative sciences, as the name indicates, involve measurement. No human measurement is perfect, and hence all measurements involve errors; so that the moment any discussion involves measurement it involves the theory of probability in a fundamental and inescapable way. In fact, all questions of properly weighting and combining discordant data, of computing probable errors or relative precisions, and of fitting curves, are questions belonging to the theory of errors. Thus all the measuring sciences have to come to the Monte Carlo mathematician for his indispensable assistance.

The kinetic theory of gases is one of the spectacular applications of pure probability. Starting with the concept of a gas as a hoard of elastic spheres, one calculates the probability of various sorts of collisions, and is led to a theory for distribution of molecular velocities,

for pressures, for viscosity, for diffusion and conduction processes, for Van der Waal's equation and for entropy and other thermodynamic concepts. Similar applications of probability lead to theories of Brownian movement, diffuse dispersion of light by molecules, double magnetic and electric refraction of fluids, opalescence, etc. As a growth out of the kinetic theory of gases there has resulted a most fertile union of probability and mechanics, this combined theory—the so-called statistical mechanics—being able to deal with such basic matters as specific heats, Nernst's Heat Theorem, thermionics, magnetic properties of gases, properties of dilute solutions, chemical kinetics, thermodynamics of stellar interiors, etc.

In connection with researches in statistical mechanics, Eddington has recently brought forward a most curious and interesting idea. This idea is profound in its philosophical implications and somewhat involved in its technical details, but its main features may be easily explained. The idea relates to our concept of time, and particularly to the unidirectional character of time. Philosophers have naturally been much concerned with the concept of time and with the fact that it sweeps ever onward and forward, never stopping and never retracing its steps. Scientists, however (at least up to the time of Einstein's 1905 paper), have had comparatively little concern with the concept of time. They have simply accepted the variable  $t$  as they have accepted other attributes of the external world. Physicists, in particular, have been curiously uninterested in the statement that  $t$  always increases. In fact, most of the fundamental theories of the physicist are completely indifferent as to whether time proceeds forward or backward. Under the equations of mechanics, for example, a planet moves about the sun in a certain definite path which it traces out

through the ages; but if one could suddenly reverse the motion of the planet, it would proceed to retrace its path, undoing day by day just what it had previously done. This illustration is typical of all those phenomena which are governed by the laws of mechanics; for in the equations expressing these laws the replacement of  $-t$  for  $t$  does not result in any change whatsoever in the form of the equations. This indifference to a reversal in time is not restricted to mechanical laws. The laws of physics may, in fact, be divided into so-called unitary laws and statistical laws. The unitary laws govern such unitary phenomena as the impact of two particles, the action of one charge on another, the emission or absorption of light by a single atom, etc. The statistical laws, as the name indicates, govern the behavior of huge assemblies of particles, charges, atoms, etc. All the unitary laws have the property just illustrated for the laws of mechanics—*viz.*, they are indifferent to the distinction between  $-t$  and  $+t$ . There has been some feeling that physics was, so to speak, falling down on the job if it could not produce satisfactory unitary laws, but had rather to take recourse to statistical laws. A statistical law states what will happen on the average, while a unitary law states what will happen in particular; and it is natural to feel that the particular statement is much the more informed and satisfactory. Therefore, physics has been primarily engaged, up until very recently, in an attempt to determine unitary laws for all phenomena, and these unitary laws, as was just pointed out, work quite as well backward as forward. That is, physics has been committed to an attitude of actual indifference toward the question of the unidirectional nature of time. In the last few years, however, physics has grown somewhat skeptical concerning the probable success of its proposal to find unitary laws for all phe-

nomena. It begins to look as if the more fundamental laws are actually the statistical laws—the probability laws—which describe the probable behavior of large groups of units.

We are now, at last, ready to consider the idea of Eddington referred to above. He points out that statistical laws are not indifferent to the distinction between  $-t$  and  $+t$ . In fact, he suggests that the unidirectional character of time—"time's arrow"—is merely our recognition of the fundamental non-reversible character of such statistical phenomena.

A consideration of one of Eddington's examples will make the idea more clear. If one takes a new deck of cards from its original wrapper, the suits are all separated and the cards are in order. If one now shuffles this deck time after time the cards become disarranged. It is an essential feature that the shuffling is applied to a large number of cards. One can not shuffle a single card; and the more cards there are, the longer can shuffling proceed without reaching completion. This shuffling process has removed a certain feature—*viz.*, the original arrangement—and it is very unlikely that any subsequent shuffling will ever reinstate the original order. Thus Eddington views shuffling, or rather the introduction of a random element by means of shuffling, as a truly non-reversible process. This non-reversibility of the process is a characteristic of phenomena relating to a large number of unitary elements or events. Unitary processes, we have seen, go backward as well as forward; but the shuffling of a statistical ensemble goes only forward. If this introduction of the shuffled character or random element into the material world is a process which can only proceed forward and never backward, then it furnishes us with a criterion for the forward passage of time. For example, suppose one has two instantaneous photographs, A and B, of a part

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of the universe, taken at two different instants,  $t_A$  and  $t_B$ . If photograph A shows a more shuffled universe than does B, then one concludes that  $t_A$  is a later instant than  $t_B$ . The illustration would be a better one if we considered not snapshots but moving pictures, for we should by rights consider shuffledness of velocities as well as disarrangements of positions. The moving picture also makes it more clear to us that it is essential that we be dealing with a statistical situation which can be shuffled rather than with a unitary process which can not. Thus, if one had a moving picture of a single swinging pendulum, he could not possibly tell whether the film were being run through normally or in the reverse direction; but if one had a picture of a swimmer diving into a pond, he could tell at once whether the direction of the action be normal or reversed. The criterion for this judgment should not, in fact, be the motion of the swimmer's body, for it is conceivable for him to be propelled up out of the water. But the motion of the "splash" and the statistical shuffling of the positions and velocities of the water particles—this is a non-reversible process which can serve as a criterion.

Eddington develops this idea and attempts to make such considerations serve as a sole and satisfactory basis for our concept of "time's arrow." Such a view-point is probably unsatisfactory to a metaphysician and is attended by grave difficulties for the physicist. Whether or not the idea turn out to be a sound one, it is, nevertheless, of great interest to us in the present connection, for it indicates fairly the sweeping significance which is assigned by modern physical theories to probability considerations.

## VI

Modern quantum theory deals with the fundamental questions of absorption and emission reactions between atoms

and radiation, making use of the wave mechanics of de Broglie, Schrödinger and Heisenberg, and the still more powerful and more abstract transformation theory of Dirac and others. This theory probably constitutes the most penetrating attack man has yet made into the delicate and minute secrets of nature. The theory is, in its presently accepted form, essentially statistical in nature, using probability concepts as its fundamental concepts. It is neither desirable nor possible to attempt to trace here in any detail just how these probability considerations enter, but it is easy to see that they do enter. In fact, all microscopic unitary processes of nature, such as the motion of a single electron or proton, the emission or absorption of light by a single atom, the impact between two particles, etc., are studied, following the Schrödinger scheme, by evaluating at every point of space and at every time a certain "wave function"  $\psi$ . In the early stages of the new theory, physicists everywhere clamored to know what  $\psi$  was. It has turned out that, say in the problem of the collision of two particles, the value of the square of this wave function at any point and at time  $t$  is the measure of the probability that one of the particles be "at" this point. This simple statement is of overwhelming significance as regards the general thesis we are defending. We are all well aware that statistical discussions lean heavily upon probability theory. But we are apt to think of such discussions as having a secondary significance. Behind the statistical or average behavior we have liked to think of the unitary processes; and we have viewed these unitary processes as strictly open-or-shut affairs, with which probability has no concern. It appears that we have no modern support for this feeling. All these unitary processes are themselves subject to chance. When a particle collides with a second, we can

not say, with contented and smug satisfaction, that the particle will after  $t$  seconds be in a certain definite spot. We have to content ourselves with saying what is the probability that it will be at the spot in question. One can, if he likes, interpret this as a failure of our attempt to find a unitary law; or he may merely say that the unitary law is itself to be phrased in the language of probability. That is an unessential matter of mere terminology. The inescapable fact is that every process that comes properly under the purview of physics is an experiment in probabilities. It means that quantitative science is a giant game of cards, and that the sort of mathematics which is most fundamentally applicable to nature is the old unsavory Monte Carlo mathematics.

## VII

Thus we have seen that the theory of probability furnishes working tools for many branches of science, the fundamental concepts for others. It seems impossible to escape from this mathematical goddess of chance. When one says that he thinks that he will be here to-morrow, the theory of probability bears on the remark from a good many angles. There are, first, the obvious chance events that might conspire to make it possible or impossible for him to be here. But the matter goes deeper than that. Let us examine the various phases of this assertion "I think I will be here to-morrow." "I think"—the theory of probability has contributed so much to the subject of the logic of inferences that one might well say that the theory of knowledge itself rests in considerable part on probability theory. In fact, Laplace, the great master of this art, said in the introduction to his "*Théorie analytique des probabilités*," "Strictly speaking one may even say that nearly all our knowledge is problematical; and in the small number of things which we are

able to know with certainty, even in the mathematical sciences themselves, induction and analogy, the principal means for discovering truth, are based on probabilities, so that the entire system of human knowledge is connected with this theory." "Will be to-morrow"—well, we have spoken of the connection between probability and the concept of the forward flux of time. "Here"—only the theory of measurement can tell one the difference between *here* and *there*. And finally, the theory of probability, as the human biologist would insist, has a good deal to do with what "I" am. To each human being is dealt out, from the two parents, twenty-four pairs of chromosomes. Using these forty-eight biological units, one can build up a number of combinations which is something over ten million times a hundred million; and all the various genetic relations in which these characterizing influences may be passed on are subject to the laws of chance. In fact, according to Guyer, "The law of probability is the fundamental principle around which biometrical investigations revolve." If we wish to push further back than our immediate parents, and if we forsake the realm of sober science, then probability plays a more whimsical rôle in determining what "I" am. If great-grandfather William had only taken along his umbrella, he would not have been soaked in that sudden cold shower, and "I" would be a different "I," with no scar tissue on my lungs. If great-great-grandfather Jonathan had only stumbled onto the right path, instead of the left, that famous time he wandered home drunk, Elizabeth would never have known, the engagement would not have been broken, and "I" would likely be tall and handsome, blond and blue-eyed.

## VIII

Thus at our birth and at our death, in our moments of whimsical fancy and

in our most serious moods of philosophic or scientific contemplation, the theory of probability plays a basic rôle. One finds it natural to ask, then, whether there is any escape from this tyranny of chance. It seems, at the outset, a fundamental paradox that one should speak of the laws of chance, since chance and law are to most of us antithetical concepts. We are up against something more than a mere paradox, however, if chance is not only to lurk always behind our shoulder ready to push us into accident, but is also to be the central feature of all our trusted and well-ordered schemes of science and society. Is there no escape from this specter of an ever-present devil shaking dice?

A few years ago I think the answer to this question was "Yes." We believed that events were reckoned as "chance" events merely because the underlying causes were so many and so intricate that man's intelligence had not as yet been able to effect a complete and rationalistic analysis. Thus the tossing of a coin was a chance event. Why was it a chance event? No one doubted that the equations of mechanics possess a unique solution. Every one thought that if we were but clever enough we could make a complete analysis of all the intricate features of muscle-tension, position in the hand, weight, form, air resistance, etc., so that the tossing of a coin would be elevated from the unrespectable level of chance events to the sacrosanct classification of good old sober, logical, cause-and-effect phenomenon.

The present view should be, I think, that this tyranny of chance, this reign of probability, is both all-inclusive and inescapable.

There are two main reasons why we can not see, at present, any probable relief from probability. In the first place, we have just mentioned that, on the older view, one always had the hope

of resolving any complex situation, apparently subject to chance, into constituent simple situations. These simple situations, one believed, would then be quite free from the taint of chance. Modern physics, however, does not take this view. It is increasingly skeptical of the possibility of always resolving the complex in terms of the simple; and even when the phenomenon in question can be analyzed in terms of unitary processes one has gained nothing. For these unitary processes themselves are chance events. *no!*

There is a second reason, moreover, why we can not just now see any escape from the tyranny of chance. This reason is based upon ideas which have found no expression above, and which we can not here expound in any detail. Heisenberg has recently announced, in his quantum theory investigation, a new principle known as the principle of indeterminism. This principle places theoretical limits upon the accuracy which we may attain in our measurements of the external world. In particular, it refers to the accuracy with which it is possible for us to make simultaneous measurement of the position and velocity of a particle. One had supposed, previously, that there was no theoretical reason why such a simultaneous pair of measurements could not be refined indefinitely; but such, according to this principle, is not the case. There is a certain minimum joint vagueness. We may, to be sure, refine as much as we choose *one* of the measurements—say the measurement of position—but if we do, there is an inescapable decrease in the accuracy with which we measure the velocity; and *vice versa*. This indeterminism is no matter of ordinary experimental error. It arises from the basic fact that we have no knowledge of the world save as we observe it. Observation is a partnership affair, active rôles being played by both observer and

observed. The observation itself, moreover, has an effect on the thing observed; and it turns out that it is never possible to reckon exactly what this effect has been.

It has not yet been made clear why this principle of indeterminism is important to the present argument. The reason is this. When one measures with a certain accuracy the position of a particle to be thus and so, we have said that he can not also measure the velocity to be exactly thus and so. The fact is that he can merely measure velocity and then conclude what is the *probability* that the velocity is thus and so. For example, if the position be measured with perfect precision, one would have to content himself with the statement that all values of the velocity were equally probable. This principle thus means that such is our relationship with the external world that all our data are of necessity probability data. This is the most fundamental consideration we have listed. If this be true, then it is unimportant to ask whether or not all

phenomena can be resolved into unitary processes, or to ask whether or not these unitary processes are themselves statistical in nature. To ask whether or not this be true is, in Bridgman's operational sense, meaningless, for the fact is that we seem condemned to observe statistically. Whether or not nature is playing a giant game of cards, our observational spectacles are such that we always see her doing so.

Science does not lay down permanent decrees, but is governed, rather, under Trotsky's principle of a permanent revolution. It is contrary to the whole accumulation of scientific experience to say that a position, occupied by science today, represents an ultimate and permanent position. There may be lurking, just around to-morrow's corner, a new and clearer view-point which will invalidate the present position. But the present position is, I believe, here fairly stated. The first part of the twentieth century (and we know not how much more) should be known as the reign of probability.

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## LUNG-FISH

By Dr. HOMER W. SMITH

PROFESSOR OF PHYSIOLOGY, BELLEVUE MEDICAL COLLEGE, NEW YORK UNIVERSITY

LUNG-FISHES are among the strangest of creatures. They are, strictly speaking, fishes, but they differ from most members of this class in possessing lungs in addition to gills, and a heart and circulation adapted to the double respiration of air and water; hence their scientific name, *Dipnoi*. The biologist is interested in them because of their extraordinary character and life habits, and the paleontologist because they are the straggling survivors of an archaic group of fishes which played an important part in the story of evolution. Once numerous and widely distributed in the fresh waters of the Paleozoic continents, they are reduced to-day to *Epiceratodus* of Australia, *Lepidosiren* of South America and *Protopterus* of Africa. *Protopterus* lives in the River Gambia, the Congo and the rivers and lakes of equatorial east Africa. There are three species which resemble each other very closely.

*Protopterus* looks like an eel, and in the aquatic phase of its life it lives very much like any other fish except that it

rises to the surface of the water regularly to breathe air. It is during the annual dry season of the tropics that the lung-fish comes into its own. Those individuals that are trapped in the swamps by the recession of the water bury themselves in the mud and pass into a state of estivation which persists until the rising waters of the "big rains" set them free again. The mud nest consists of a burrow extending from twelve to eighteen inches underground. This burrow is formed by the repeated journeys of the fish to the surface to get air while the mud is soft. As the last water dries out of the burrow the fish curls itself in a close coil at the bottom with its head pointed up the burrow, and slowly passes into a state of sleep that approaches suspended animation. The estivating fish covers itself with a parchment-like cocoon formed of dried slime secreted from its dermal glands. This cocoon completely envelops the animal except at the mouth, into which it extends as a short, open tube and through which



FIG. 1. THE FINS OF *PROTOPTERUS* ARE DEGENERATE FILAMENTOUS APPENDAGES, USELESS FOR CRAWLING OVER LAND IN SPITE OF ITS ESTIVATING HABITS AND AERIAL RESPIRATION.

respiration is carried on. So tightly is the lung-fish encompassed by the cocoon and adjacent mud that there is not the slightest room to move; it is just as effectively imprisoned in the hard mud as though it were buried in concrete. There is probably no other animal in the world that becomes completely immobilized for such long periods of time. There is no possibility, of course, of obtaining food in its mud prison, and it is forced to live upon such fat as is stored in its body and upon the body tissues themselves.

The natives of Africa like to eat the lung-fish. They catch them in nets in Lake Victoria and in basket traps in the swamps and streams. What is probably the largest lung-fish on record was caught by native fishermen in Lake Victoria. This specimen is preserved in the Nairobi Museum of Natural History. It is seven feet long and has a head, if memory serves us, which must be twelve inches across. This fish was probably over one hundred years old. During the dry season the natives also dig the estivating fish out of the mud. They search the water-courses and swamps for the burrows, and thrust a stick into every suspicious looking hole. If the end of the stick smells of fish on withdrawal, the helpless victim is disinterred.

In years past numerous small specimens of estivating lung-fish have been dug out in Gambia with the mud nest intact and shipped to England, Germany and the United States. Some of these have been awakened from estivation by immersion in water, and kept in aquaria for years.

Through the favor of the John Simon Guggenheim Memorial Foundation we collected a number of active lung-fish from Lake Victoria during the summer of 1928 and brought them alive to New York. Some of them were induced to enter into estivation by placing them in mud which was allowed to dry out at

room temperature. At the end of twelve to fifteen months a few specimens were disinterred for physiological study.

Since we did not wish to waken them we did not put the estivating lung-fish into water, but instead carefully cut away the rock-like walls of the nest and removed the animal with as little disturbance as possible. The lung-fish were in a state of profoundest sleep or inhibition. This sleep was not dispelled by handling them, and they could be kept in a metabolism chamber for days or weeks without awakening. When placed in water, however, they are wakened in a few hours, probably by asphyxia, for they are unable to breathe without rising to the surface for air. The sleep is apparently of a nervous rather than chemical origin and appears to be induced by the prolonged immobilization of the animal in its rigid prison. We know that all the higher animals sleep at some time, and that this sleep has come to be a period of physical and nervous rest that is necessary for continued health and activity. This is probably true of all animals. We know that insects sleep, sometimes so profoundly that they can be picked up without being wakened, while other animals, such as wild mammals, birds, etc., sleep so lightly that they are usually alarmed into full activity by our approach. There are possibly some fish that sleep while swimming (mackerel), but others hide under stones, among grasses or beneath the sand during parts of the day or night, and remain physically inactive for considerable periods of time.

It is obvious that certain conditions are usually necessary for sleep in the higher animals, such as the absence of unusual sounds and other external stimuli. But there are also important internal factors that may work actively to induce the sleep. It is not yet clear to what extent this condition is due to

<sup>1</sup> *Journ. of Biol. Chem.*, 1930, 88: 97-130.

simple passivity in the higher nervous centers or to active inhibition of these centers. But the lung-fish, we may believe, lying perfectly still in its mud prison for long periods of time and shut off from all external stimuli, probably goes to sleep for much the same reason as the higher animals. But unlike other fish, the sleep is deep and not easily dispelled; it is more like that of a very tired child, quite indifferent to the outside world. When the floods again cover the land the lung-fish is quickly awakened, and breaking out of its cocoon, slips to the top of its burrow and swims free.

When the lung-fish buries itself in the mud it does not know how long it is to be imprisoned. Though the tropical rains come in an unfailing annual cycle, the high-water level varies from year to year and a particular piece of ground may remain dry for several years. It is to be expected, therefore, that the lung-fish would conserve its energy stores to the greatest possible extent. This is effected in part by the fact that the animal is forcibly restrained from any muscular activity, except such as is required for respiration and the circulation of blood. But there still remains the residual metabolism of the living tissues which continue to idle, so to speak, even while the animal is quiet and asleep. This fraction constitutes about four fifths of the total metabolism of the active, starved lung-fish. During estivation this residual metabolism is decreased, not abruptly, but gradually and apparently in proportion to the development of emaciation. Stored fat and tissue must furnish energy for the maintenance of life, and as these fuel-stuffs are burned the fasting animal loses considerable weight—about 25 per cent. during the first year. Since the rate at which the body fires burn decreases as the animal becomes more and more emaciated, it appears that an additional and equal amount of tissue will last at least



FIG. 2. A FIFTEEN-INCH LUNG-FISH IS SEEN CURLED AT THE BOTTOM OF A BLOCK OF MUD. THE COCOON HAS BEEN PARTLY TORN OFF IN CHIPPING AWAY THE HARD MUD. THE BURROW EXTENDS BACKWARD IN THE PICTURE. THIS FISH HAD BEEN IN ESTIVATION FOR FIFTEEN MONTHS.

twice as long. It may reasonably be expected that the estivating lung-fish can live for three to five years, and possibly longer. (There is no reason to believe that any other estivating or hibernating animal, not excluding horned toads, could live longer.)

One very interesting aspect of the estivating state is that little or no water is available. Water is constantly being lost by evaporation from the lungs as air is inhaled and exhaled. There is therefore an imperative need to protect the body from loss of water through the skin by the impervious cocoon, and to conserve this precious fluid otherwise to the utmost degree. For this reason urine excretion is completely suspended. All the non-volatile waste products formed from the combustion of the tissue pro-

teins, etc., which are normally excreted by the higher animals as fast as they are formed, are allowed to accumulate in the blood and tissues. Urea is the most important of these waste products and this substance accumulates in the body in relatively enormous concentrations. When the lung-fish is returned to water and resumes active life the accumulated metabolites are discharged through the kidneys and gills, about twelve to fifteen days being required for their complete excretion.

The concentration of urea in the estivating lung-fish rises to values (2 per cent. of body weight or better) greater than are known to occur in any other animal except the sharks and skates. In these the urea is an integral part of the blood and plays an important physiological rôle. There is, nevertheless, no evidence that the lung-fish is poisoned by the "uremic" state that occurs during estivation. It is well known that urea itself is not very toxic, and apparently any other injurious metabolites which might be quickly excreted by other animals are converted by the lung-fish into innocuous substances.

All things considered the lung-fish appears to rank among the hardiest of living creatures. *Protopterus* can survive in foul pools the temperature of which rises above 100° F. in the tropic sun, and they are completely independent of the respirable quality of the water in which they live; they can live for long periods without food and with complete cessation of kidney function; and though they can not live for more than a few hours under water, they can, paradoxically, live out of water—and without water—for months or years.

The way in which the lung-fish uses

its lungs during the dry season recalls the probable circumstances under which air-breathing was first evolved. It is fairly certain that air-breathing was practiced by fishes long before any sort of efficient legs for crawling over land had appeared. The first air-breathers were apparently the ganoid fishes which lived in the early Devonian period. It has been concluded from several lines of evidence that climatic stress played the principal rôle in this important evolutionary step. The period in which the first air-breathing vertebrates were evolved was characterized by very arid conditions, broken only by occasional rains. The marked alternation of wet and dry seasons caused the masses of fresh water on the continents, widespread at flood time, to shrink and become foul during the ensuing dry season. The fishes trapped in the pools were forced to make greater and greater use of air. Thus the first lungs came into existence as adaptations to meet these difficult climatic conditions. Through one branch of the Devonian fishes these lungs were passed on to the ancestors of the terrestrial vertebrates, and through another branch to the higher fishes. But the latter have, for the most part, let the air-breathing apparatus fall into disuse, and in most of the modern fishes only a vestige remains in the closed-off air-bladder, or it has disappeared entirely. In the lung-fish, however, the primitive lung persists as such, and we may imagine that the surviving members of this group use it in much the same way as did their Paleozoic ancestors. In this view, the life habits of these fish show us how the vertebrates were freed from an aquatic life and set upon a course of terrestrial evolution.

## THE PROGRESS OF SCIENCE

### THE TOTAL ECLIPSE OF THE SUN

THE second total eclipse of the sun for 1930 occurred on October 21-22. The path of totality extended for nearly 7,000 miles across the South Pacific Ocean in a southeasterly direction. The eastern end of the path terminated in southern Chile just as the sun was setting. The width of the shadow was about fifty miles in the middle of its course, but narrowed down to a band of less than half this width at its extremities. The duration of the eclipse was only ninety-two seconds and it took place a little after nine o'clock in the morning on Niuafoou Island, which was about four o'clock in the afternoon, eastern standard time.

For most favorable observation it was necessary to choose a position in the South Pacific Ocean where the sun was not too near the horizon at the time of the eclipse. This condition was met on Niuafoou, the little island from which the astronomical observations were made. This bit of land in the Tonga group is

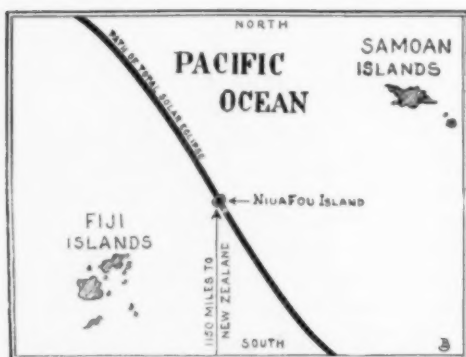
volcanic in origin, with a contour that is nearly circular, and is about three miles in diameter. A large central area nearly a hundred feet above sea level consists of a lake of brackish water. The island is under British protection and its population is composed of about 1,500 Polynesians and a couple of white traders.

The only important article of commerce is an exceptionally large variety of cocoanut which has never been successfully grown elsewhere. There is no good harbor and, as the shore is precipitous, the facilities for landing large packages are very poor. Mail for inhabitants is transferred between the island and the monthly inter-island steamer in sealed tin cans which are conveyed by swimming natives.

The United States Naval Observatory sponsored an expedition to make observations of the eclipse. Commander C. H. J. Keppler was in administrative charge. He was also in charge of the



THE LAKE ON NIUAFOU ISLAND



Monthly Evening Sky Map  
THE PATH OF THE TOTAL SOLAR  
ECLIPSE

successful expedition to Iloilo, P. I., in May of last year. Lieutenant H. C. Kellers, of the Medical Corps, was placed in charge of the health of the expedition.

Dr. S. A. Mitchell, director of the Leander McCormick Observatory at the University of Virginia, was scientific director of the party, and in direct charge of the spectrographic work on the flash spectrum. The personnel of the expedition was further made up of the following scientific men: Professor R. W. Marriott, of Swarthmore College, in charge of the large-scale photographic work on the sun's corona and of the photographs for the further study of the Einstein effect; Dr. J. J. Johnson, of the California Institute of Technology, in charge of photometric observations; Dr. Weld Arnold, of the American Geographical Society, Mr. B. P. Sharpless, of the Naval Observatory, and Mr. H. Fales, of Pasadena, assisted



NIUAFOU ISLAND

IN THE SOUTH PACIFIC OCEAN FROM WHERE THE ASTRONOMICAL OBSERVATIONS WERE MADE. IT IS OF VOLCANIC ORIGIN AND ABOUT THREE MILES IN DIAMETER.



A SCENE NEAR THE POST OF OBSERVATION

in carrying out the extensive program of observation; Dr. T. A. Jaggar, of the Hawaiian Volcano Observatory, accompanied the expedition to study the volcanic and seismic conditions. There was a serious volcanic eruption about two years ago, and such eruptions are said to occur about once in fifteen years. A New Zealand party of six astronomers joined the expedition on the island.

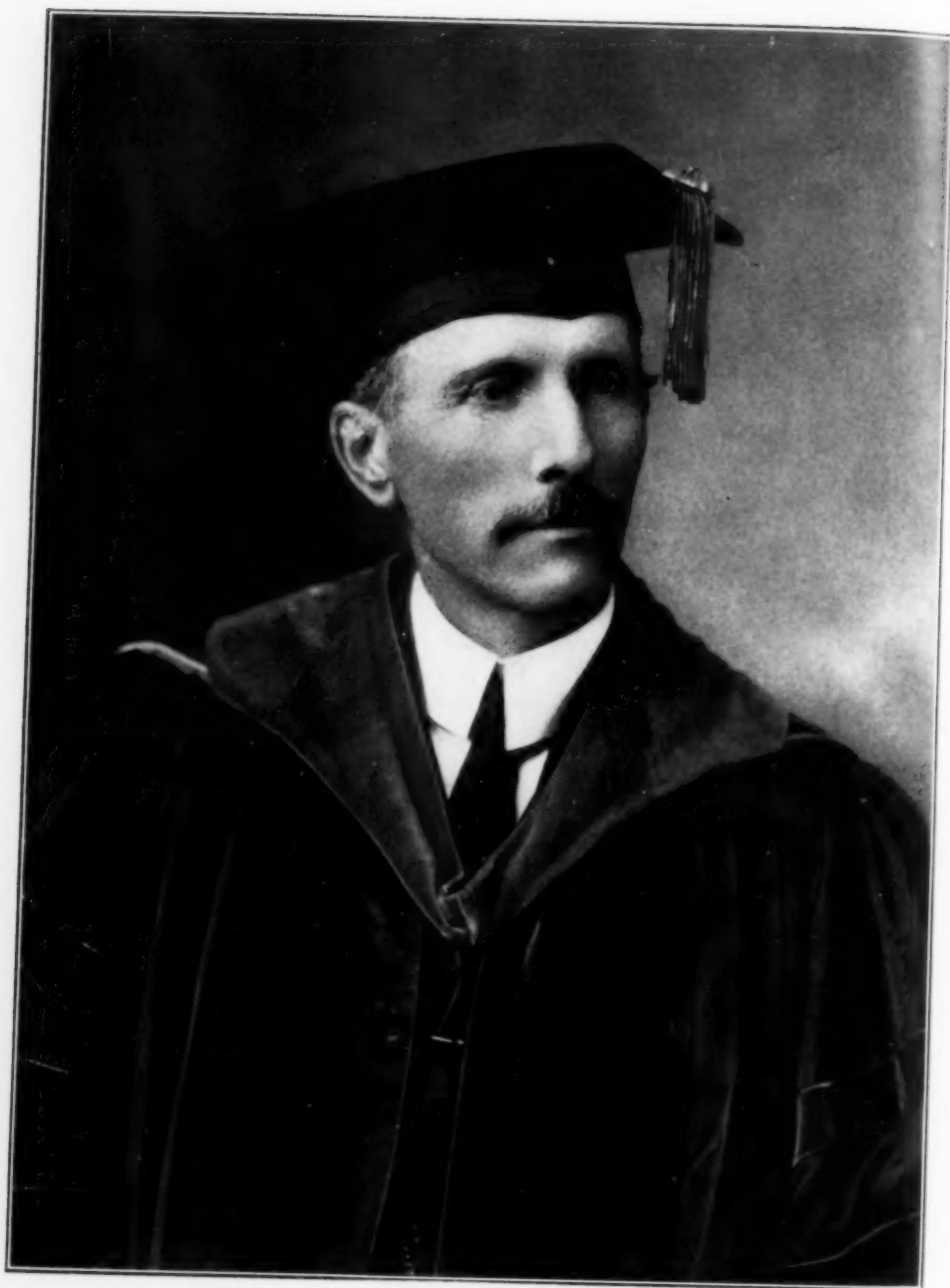
In addition to the scientific staff eleven enlisted men of the Navy and Marine Corps assisted in construction work and in taking observations. These men, selected from the personnel of the Battle Fleet, sailed from the Mare Island Navy Yard aboard the mine sweeper *Tanager* on June 25 and arrived at Samoa on August 9. Enlisted men included a rigger, an optical repair and instrument man, a carpenter to construct large cameras, an expert photographer, two general assistants, two radio operators, two cooks and an interpreter.

About 115 boxes and cases of scientific instruments and equipment were conveyed to the island, besides camp equipment and food supplies sufficient for twenty men for sixty days and about

8,000 board feet of lumber for the construction of various cameras. The largest of these cameras had a focal length of sixty-five feet and included a photographic developing room.

The objectives of the expedition included the following: a series of spectrographs including those of the flash spectrum; a series of photographs of the inner corona with the 63-foot camera, of the middle and outer corona with smaller cameras, moving pictures of the whole eclipse, including partial phases and totality, a special series of photographs of the star field near the sun for the corroboration of the Einstein effect, a series of photographs for photocentric determinations of the light of the corona, and observations for the times of contacts and of the shadow bands. The latitude and longitude were more accurately determined, and meteorological, volcanological, seismological, zoological and botanical data were secured as by-products.

The brief period for astronomical observation made it necessary for the observing parties to rehearse every movement with great care in advance of the eclipse.



FLORIAN CAJORI

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## FLORIAN CAJORI

IN the death of Florian Cajori, of the University of California, America has lost one of its most distinguished contributors to the history of science. THE SCIENTIFIC MONTHLY had the privilege of printing many articles by him, the last of these being on Johannes Kepler in the May number of the present year.

In an article printed in *Science* Professor David E. Smith states that Cajori was born at St. Aignan, near Thuisis (Graubünden), Switzerland, on February 28, 1859, and came to the United States at the age of sixteen. Entering the University of Wisconsin, he received the degree of B.S. in 1883, spending the year 1884-1885 in graduate work at the Johns Hopkins. He then went to Tulane University (1885) as assistant professor of mathematics, becoming professor of applied mathematics two years later (1887). In 1889 he went to Colorado College as professor of physics, subsequently taking the chair of mathematics (1898-1918) and becoming dean of the department of engineering (1903-1918). During all these years he paid particular attention to the history of the subjects of his major interest, and in recognition of his work in this field he was called to the University of California in 1918 as professor of the history of mathematics, a unique title either in this country or abroad. This position enabled him to devote his time largely to research and writing, and the result amply justified the action of the university in creating the position, and his own decision in accepting it.

Forty years elapsed from the date of the publication of his "Teaching and History of Mathematics in the United States" (1890) to the time when death compelled him to lay aside the work which he had hoped to complete—an edition of Newton's "Principia." Dur-

ing these years his contributions to the history of mathematics, physics, geodesy and astronomy were numerous and of increasing value. Besides writing a large number of articles and making a brief excursion into the text-book field, he wrote the following historical works: "History of Mathematics" (1894, with a revised edition in 1919), "History of Elementary Mathematics" (1896, with a revised edition in 1917), "History of Physics" (1899), "History of the Logarithmic Slide Rule" (1909), "William Oughtred" (1916), "History of the Concepts of Limits and Fluxions in Great Britain from Newton to Woodhouse" (1919), "The Early Mathematical Sciences in North and South America" (1928), "The Chequered Career of Ferdinand Rudolph Hassler, First Superintendent of the United States Coast Survey" (1929) and the work by which he will chiefly be remembered—"The History of Mathematical Notations" (2 volumes, 1928, 1929).

His work was duly recognized by learned societies and by various colleges and universities. He was a member of the American Mathematical Society, the Mathematical Association of America, the Deutsche Mathematiker-Vereinigung, the Mathematical Association (England), the American Academy of Arts and Sciences and the American Association for the Advancement of Science, holding offices in at least two of these societies. He was honored by the degrees of Ph.D. (Tulane, 1894), LL.D. (University of California, 1912, and Colorado College, 1913) and Sc.D. (Wisconsin, 1913). As the leading historian of mathematics in this country, his loss will be deeply felt by all who have an interest in this important field of learning.



DR. ARTHUR HARDEN

PROFESSOR OF BIOCHEMISTRY AT LONDON UNIVERSITY AND HEAD OF THE DEPARTMENT OF BIOCHEMISTRY AT THE LISTER INSTITUTE, WHO SHARED WITH PROFESSOR HANS VON EULER THE NOBEL PRIZE IN CHEMISTRY FOR 1929.



GEORGE WESTINGHOUSE  
1846-1914

#### THE GEORGE WESTINGHOUSE MEMORIAL

ALTHOUGH not primarily a scientific man, for it is as a great creative and constructive genius who rendered commercially practical and beneficial to humanity the scientific developments of himself and of others that he won his greatest recognition, George Westinghouse was a pioneer in many scientific fields.

The George Westinghouse Memorial, recently erected in Schenley Park, Pittsburgh, and dedicated in October, depicts six of Mr. Westinghouse's most notable achievements, though he patented over 400 inventions.

In 1861, at the early age of 15, he invented a rotary engine; and at 20 he had produced his first railway inventions, an appliance to replace derailed railway cars and the reversible steel railway frog.

When 21, he had patented the air brake, the application of pneumatic pressure to separate brake units on each car of the train, the entire system being operated by the engineer in the locomotive cab. This invention, which made rapid rail transportation safe, revolutionized rail traffic throughout the world and promoted the development of the great



THE GEORGE WESTINGHOUSE MEMORIAL IN SCHENLEY PARK, PITTSBURGH

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THE CENTRAL PART OF THE MEMORIAL

railroad facilities of to-day. But the struggle to obtain for his great invention the recognition it deserved and the final success of this fight against almost overwhelming odds shows the courage, the perseverance, the organizing ability and the unquenchable spirit of George Westinghouse. In 1868 he founded the Westinghouse Airbrake Company to manufacture and sell the airbrake.

In 1870, Mr. Westinghouse designed and built a jet steam turbine. In the same year he traveled to Europe to begin a ten-year crusade in England and on the continent to establish his railway safety appliances. In 1873 he designed and built the first automatic central telephone exchange, which was, unfortunately, a score or more years ahead of

the times. In 1879 he invented the principle of modern railway signalling, the pneumatic system of interlocking signals, operated by compressed air.

His mind was not entirely wrapped up in blue prints and patents; at this period of his life he delved into a social experiment—possibly the first effort in America to better the workman's living condition—by instituting the Saturday half holiday.

The year 1880 saw him terminate his inventive activities for a brief period and return to organization of industries. In that year the Westinghouse Machine Company (later reorganized as the present Westinghouse Electric and Manufacturing Company) was founded in Pittsburgh for the manufacture of high

speed engines designed by H. H. Westinghouse, George's brother. The next year the Union Switch and Signal Company, of Pittsburgh, was formed for the production of the pneumatic switch and signal.

The almost unlimited supply of natural gas located in the hilly country of Western Pennsylvania was seen by the alert inventor to be of potential value to industries and homes if it could be piped to consumers. With his customary, lightning-like thoroughness it was but a short time after finding a use for gas that he had perfected a complete system for transmitting gas through pipes, invented a meter for measuring consumption and organized companies to distribute this gas.

The year 1885 marks a new era in the life of Westinghouse; in that year he began investigating the little-explored realm of electrical application, and decided that alternating current, then far from the practical tool it is to-day, would provide the solution for the problem of the efficient and flexible transmission of power. He purchased the transformer patents held by the Englishmen, Gaulard and Gibbs; and in 1886 he organized the Westinghouse Electric Company for the manufacture of electric lighting apparatus. He then engaged Nicola Tesla, a young European scientist of great talent, who, in collaboration with Westinghouse, developed the alternating current induction motor.

As the champion of alternating current, Mr. Westinghouse met hostile and

stubborn opposition, but his indomitable character finally won recognition of the advantages of this type of current. Lighting the Chicago World's Fair, the construction of the world's first hydro-electric plant at Niagara Falls, the design of the early street cars and the production of the first electric locomotive stand as milestones along the path he trod as he achieved success in the electrical industry.

To his well-filled record of achievements were added the first commercial steam turbines in 1898; the erection of the British Westinghouse Company at Manchester, England, and the application of the steam turbine to steamships, in collaboration with Rear Admiral George Melville, U. S. N., and John H. MacAlpine, inventor, in 1904. He began development of the air spring for automobiles in 1910; made a successful installation of reduction gears in the United States collier, *Neptune*; and in the year of his death, 1914, he had the final honor of having his reduction gears selected by the United States Navy for installation in two battleships. His machinery was chosen in preference to any other offered.

In 1914 death cut short further research and further achievements of one of the foremost contributors to America's industrial supremacy. The world knew him best as a remarkable industrialist and a developer of electricity, but, in addition, admired and respected him as an engineer, inventor and man of science.